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Richard Rokey M.D.

THE
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OF
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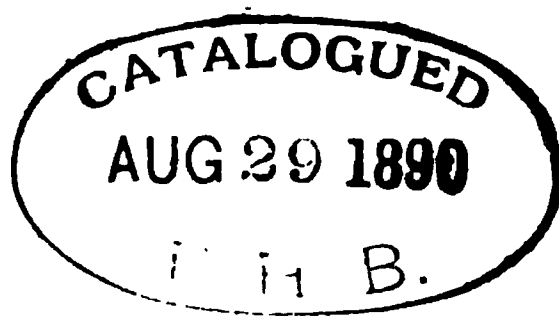
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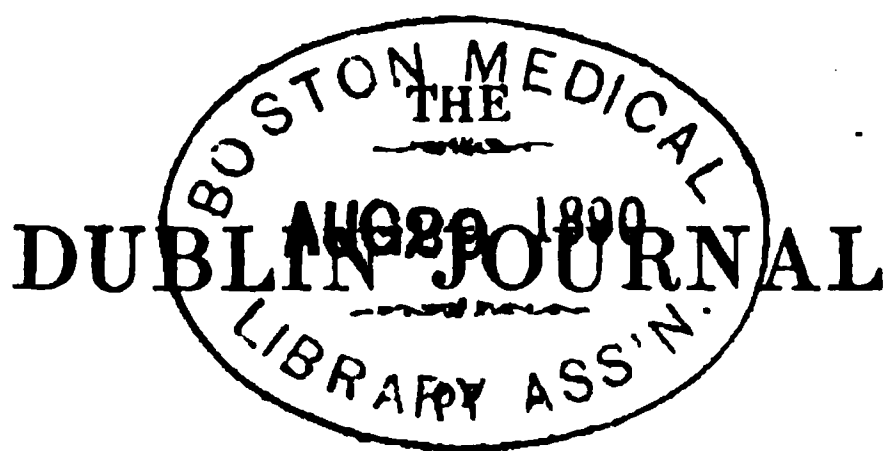
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MEDICAL AND CHEMICAL SCIENCE.

1 MARCH, 1834.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*On some Compounds formed by the Action of Chloride of Platinum and Chloride of Tin.* By ROBERT J. KANE, M. R. I. A., Professor of Chemistry to the Apothecaries' Hall of Ireland, Corresponding Member of the Societies of Pharmacy and of Medical Chemistry, of Paris, &c. &c.

AN accidental observation of the intensely red colour produced by mixing solutions of proto-chloride of tin and of per-chloride of platinum, induced me to examine into the nature of their mutual reaction at considerable length. I had very nearly reached the termination of my labours, when the records of them were, by some accident, mislaid; and except so far as my memory could serve, I lost every corrected statement of those results, to obtaining which I had devoted a space of many months. I resolved to recommence the investigation at a future period. I have subsequently attempted to put that resolution into practice, but have not been able to devote to it the necessary time. Still less do I now see any probability of having at my disposal

sufficient leisure ; and have therefore resolved, as I cannot again myself complete this train of research, to bring forward such a general account of the novel phenomena that I observed, as may have the effect of inciting others to the task. I fortunately possessed in the laboratory book such accounts of the experiments as may be sufficient for that purpose.

A solution of the chloro-stannite of ammonium (first described by my friend Dr. Apjohn) was placed to evaporate in an unglazed biscuit ware capsule, in which there had been many weeks before chloride of platinum. The tin solution gradually became coloured red, and seeking for the cause of this, it appeared that the trace of platinum, which the dish had absorbed, was sufficient to colour intensely the solution of the chloro-stannite. To verify this rationale I mixed together solutions of proto-chloride of tin and per-chloride of platinum. A splendid red colour was immediately produced. To determine the nature of the body formed by the union of the two chlorides, and which possesses this brilliant tint, was the object of my future investigations.

Per-chloride of platinum and crystallized proto-chloride of tin were dissolved separately in small portions of water, and the solutions mixed : they became instantly of an intense wine red colour. This red liquor was divided into two equal portions ; one portion was evaporated very cautiously nearly to dryness, when on cooling it solidified into a mass of intensely deep red crystals, very deliquescent, and staining the skin brown. They were soluble in a small quantity of water, in alcohol and in ether ; these solutions were red. When they were evaporated the substance again separated in the crystalline form. It crystallized more readily from its etherial than from its alcoholic or watery solution.

These crystals are in plates, presenting an appearance like *moirée metallique*. When heated they give out much water acidulated with muriatic acid, (they had not been well dried), some chloride of tin, and finally, after having been subjected

to a full red heat, metallic platinum and some peroxide of tin remains behind.

Although soluble in a small quantity of water, this substance is totally decomposed by a larger quantity; a red powder separating, which, when dried, becomes brown. These crystals are soluble in muriatic acid; the solution is reddish brown, and contains proto-chlorides of tin and platinum; the addition of potash not producing the separation of any chloro-platinate of potassium.

These crystals are, as was mentioned before, decomposed by much water: in order to examine the products of this decomposition, a quantity of the crystals was mixed with a large quantity of water; a considerable quantity of red precipitate immediately fell: this was separated by the filter.

The liquor was acid. It contained free muriatic acid, a mixture of proto-chloride of tin, and much per-chloride. It did not contain any platinum.

The precipitate in drying became of a brown colour; it had not any trace of crystalline structure. It was totally indissoluble in water. When heated it gives out a white vapour which reddens litmus paper, and condenses into a white solid matter (chloride of tin). Metallic platinum, mixed with some peroxide of tin, remains behind.

When acted on by nitric acid it forms a yellow liquor, from which per-oxide of tin is separated. When digested with muriatic acid it dissolves, forming a fine red solution, very similar to the original one. When acted on by an alkali, a black powder is formed. We shall recur to the detail of these reactions presently.

In order to analyze this powder, the following method was pursued:

Twenty grains of the red powder were boiled in water of ammonia until it was converted totally into the black powder. The water of ammonia was then neutralized, and nitrate of silver added. Chloride of silver was thrown down, which when

dried weighed 18.2 grains, equivalent to 4.48 grains of chlorine.

Twenty grains of the red powder were boiled in aqua regia until they dissolved entirely. The solution was then evaporated nearly to dryness. Nitric acid was added to it in excess and it was again evaporated: when nearly dry a quantity of water was poured on it, when a considerable quantity of per-oxide of tin was precipitated, which, separated by the filterer and ignited, weighed 12.2 grains, equivalent to 9.6 grains of metallic tin.

The liquor from which the tin was thus separated contained all the platinum as per-chloride. It was considerably concentrated and then mixed with sal-ammoniac and alcohol. The scarcely soluble chloro-platinate of ammonium which was thrown down having been collected, weighed 10.7 grains, equivalent to 8.13 grains of bi-chloride, or to 4.73 of metallic platinum.

By this analysis there were obtained from twenty grains of the powder,

Chlorine	=	4.48
Tin	=	9.60
Platinum	=	4.73
		<hr/>
		18.81
		<hr/>

The platinum existed evidently as proto-chloride, and it is very probable that the rest of the chlorine existed in the state of proto-chloride of tin. Now the platinum, united with 1.69 grains of chlorine to form proto-chloride, and the remaining 2.79 grains of chlorine converts into proto-chloride 4.64 grains, or very nearly exactly one half of the tin. In what state of combination was the other half? Let us call to mind that when proto-chloride of tin is decomposed by much water, a white powder falls which contains proto-oxide and proto-chloride of tin, the tin being evenly divided between the two. It is evident that the proto-chloride of tin, existing in the crystalline substance, was decomposed by the large quantity of water, and

we consequently obtained as much prot-oxide as proto-chloride of that metal in the precipitate.

Arranging, therefore, the results of the analysis on this principle, we shall have

4.73 Platina	4.73 Platina	}	= 6.42 Chloride of platina.
	1.69 Chlorine		
4.48 Chlorine	2.79 Chlorine	}	= 7.43 Chloride of tin.
	4.64 Tin		
9.60 Tin	4.96 Tin	}	= 5.65 Oxide of tin.
	.69 Oxygen		
<hr/> 18.81			
			<hr/> 19.50
			.50 loss.
			<hr/> 20.00
			<hr/>

Now calculating the atomic relations existing between these proportions, we find them approximating very closely to

1 atom proto-chloride of platina	= 134.20
1½ atom proto-chloride of tin	= 121.45
1½ atom pro-toxide of tin	= 100.35
	<hr/> 356.00

and its formula,



On that supposition the precipitate would contain in twenty grains,

Proto-chloride of platina	= 7.56
Proto-chloride of tin	= 6.82
Pro-toxide of tin	= 5.62
	<hr/>

There is a slight deficiency in the quantity of platina detained by experiment, and a trivial excess in the quantity of proto-chloride of tin. But the coincidence is sufficiently close to warrant us in considering such to be the real composition of this body.

This substance is soluble in muriatic acid. The solution is of a fine red colour, like the original liquor from which it had been precipitated. It is not decomposed by water if the muriatic acid be considerably in excess. This solution, when evaporated, gives a soft crystalline mass, very acid, and resolvable in water. These crystals were evidently composed of the chloro-platinite of tin, united to chloride of hydrogen (muriatic acid), and were evidently analogous to the oxygen salts which crystallize with oxide of hydrogen (water) in combustion.

When this acid solution is mixed with solution of corrosive sublimate, a very interesting effect is produced. The brown powder is again thrown down, the corrosive sublimate uniting with the muriatic acid. If the solutions be boiled together, a different effect is produced; the proto-chloride is converted into per-chloride of tin, while the corrosive sublimate loses either one-half or the whole of its chlorine.

We know that the proto-chloride of platinum and the proto-chloride of tin are both slightly negative, and their compound should therefore act feebly as a chlorine acid. Hence it unites with the positive chloride of hydrogen, forming a crystalline compound; from which, corrosive sublimate, being a much stronger acid, takes away the base; or we may suppose the muriatic acid to combine separately with chlorides, and a mere mixture of chloro-stannite and of chloro-platinite of hydrogen to exist in solution. Which ever view we adopt will have many analogous cases amongst the oxygen combinations. The former is that which I prefer in consequence of the red colour which a mere mixture should not possess.

By the action of much water the crystals are decomposed into this reddish brown powder and muriatic acid. From the analysis of the powder I therefore consider the crystals to be composed of one atom of proto-chloride of platinum, and three atoms of proto-chloride of tin. Its formula is $(\text{Ch.} + \text{Pt.}) + 3(\text{Ch.} + \text{Sn.})$

When ammonia acts on the crystals or on the red powder, it

takes away the chlorine, and as much water is decomposed as converts the metals into prot-oxide and the chlorine into muriatic acid. Sal-ammoniac exists in the liquor, and there remains a jet black brilliant crystalline powder, which consists of platinum and tin, both in the state of pro-toxide.

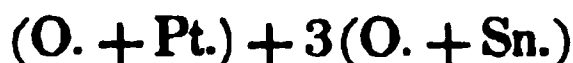
This substance is insoluble in water, soluble in muriatic acid. The solution is olive coloured, and containing platinum and tin, both in proto-combination. This solution is decomposed by water except it contains a considerable excess of acid. The precipitate is of a dirty white colour. Solution of sal-ammoniac did not dissolve this powder. It was not acted on by solution of nitrate of silver or of ioduret of potassium.

When heated nearly to redness there appeared a sudden scattering, as if from an explosive disengagement of gas, and immediately afterwards it suddenly glowed intensely as if burning like tinder. During this combustion no gas was disengaged. When the process was carried on in close vessels there was a slight absorption of oxygen, but the combustion took place, though less completely, in hydrogen and carbonic acid. After this ignition the powder preserved all its external character. It was, however, then insoluble in muriatic acid, and soluble in aqua regia. The solution contained per-chlorides of platinum and tin.

It is easy to explain this phenomenon. The black powder is a combination of prot-oxide of platinum and prot-oxide of tin. The former substance is decomposed at a low red heat, giving out oxygen in which the prot-oxide of tin burns. The oxide of platinum does not yield enough of oxygen to convert the whole of the prot-oxide of tin into per-oxide, and therefore the combination is less brilliant in carbonic acid than in atmospheric air.

The ammonia, by acting on the brown powder, takes away nothing but muriatic acid; the metals remain in the same proportions as in the brown powder, but both in the state of prot-oxide. If, therefore, the analysis given above of that powder

be correct, this brilliant black crystalline substance is composed of one atom of prot-oxide of platinum, and three atoms of prot-oxide of tin. Its formula is



When this crystalline powder is digested in solution of potash it loses its brilliant appearance and becomes dull. The liquor contains prot-oxide of tin. The dull black powder is prot-oxide of platinum. When the reddish brown powder, or the red crystals are digested in solution of potash the same effect is produced; the chlorine and oxide of tin being dissolved, and the oxide of platinum alone remaining behind. If the potash be considerably in excess, it takes up a quantity of the oxide of platinum, and becomes greenish coloured. This oxide of platinum dissolves rapidly in muriatic acid.

I hope that the above description of these compounds may be found sufficiently interesting to lead to a more perfect investigation. It must be imperfect to a certain extent, and I only bring the subject now forward that it may be more completely investigated by others.

ART. II.—*Report of a Case of Urinary Calculi, containing Human Teeth, removed from the Female Bladder.* By GEORGE WILLIAM O'BRIEN, M. D., Licentiate of the Royal College of Surgeons in Ireland, and Surgeon to the Clare Infirmary, Ennis

MARY MAC MAHON, a labouring woman, aged fifty years, was admitted into the county of Clare infirmary, on the 9th of October, 1833, suffering from symptoms of stone in the bladder. She complained of severe pain, and appeared much harassed and reduced by the complaint.

The account she gave of her previous state of health was not very satisfactory; from her statement, however, it appeared

that about four months ago she became affected with acute pain in the loins ; this, however, gradually diminished ; but was succeeded about six or seven weeks since, by difficulty in making water. The desire to evacuate the bladder is now constant, and the pain felt on attempting to do so quite excruciating, especially after the bladder has been completely emptied. The flow of urine is at times altogether interrupted ; and the pain becomes then most severe until the evacuation is again restored. For the last three days she has had complete retention. Irregular shivering fits, followed by copious cold sweats and by loss of appetite and rest, have contributed to reduce her to a very helpless state.

Upon sounding the bladder, a calculus can be distinctly felt, and seems to be of considerable size. She states herself, that once or twice, the stone came so close to the orifice, that she has been able to scratch some of it off, with her finger. The necessity for attempting to remove the stone having been determined on, the patient was placed in the sitting posture on a chair, the legs being held asunder by an assistant. A small forceps was now introduced, with which, after much trouble, the calculus was seized ; but having broken on account of its brittleness, it again slipped away from the instrument. The forceps having been once more introduced, the calculus was with great difficulty secured between its blades. An effort was now made to bring it through the urethra, but after a long trial, it altogether failed. Finding that the stone could not be got thus to pass, a small incision (about a quarter of an inch long) was made, with a blunt pointed bistoury, in the anterior part of the urethra, as being the most convenient direction. On this being done, the calculus was readily extracted. It was of an oval shape, with its sides much flattened, and one of them smooth as if it had rubbed against another stone. From the opposite side appeared a projection about a quarter of an inch in length, presenting a striking resemblance to a hu-

man tooth, with the fang turned outwards. On clearing away the calculous matter from around this projecting body, it proved to be indeed a human tooth ; one of the molars possessed of a perfect covering of enamel.

During the operation the patient fainted, and continued so weak that it was necessary to remove her immediately to bed. A full opiate was administered and she remained tranquil for a few hours ; at the end of this time, however, another paroxysm of pain came on, and after much suffering another calculus came away. From this time she got complete and permanent relief. This second calculus was of an egg shape, somewhat larger than the first, and quite smooth. On examination it had a glossy appearance at one end, which on being scraped presented the extremity of another tooth.

The patient slept well on the night after the operation, (which she had not done for some time before,) and she expressed herself as having been greatly relieved. The urine at first flowed freely and constantly through the wound, which, however, had completely healed in a week. Her amendment was now in every respect rapid. She could retain her urine perfectly, and her general health and appearance were much improved.

In reply to our inquiries, she could give no account of the teeth which had been found in her bladder, nor had she any idea of their being there ; but mentioned that all her teeth had been loose for the last thirty years ; since she had taken mercury for a venereal complaint, communicated by her husband. A number of her teeth had from time to time fallen out ; some having been found in her bed, which had dropped out at night, while she was asleep ; but she could not say that every one had been found that had dropped out. She had not had any serious illness from the time she had taken mercury until the present attack ; and at no time did she appear to have had any complaint like inflammation in the abdomen. We are therefore left wholly to conjecture, in attempting to account for the novel

phenomenon, of human teeth being found to constitute the nuclei of stone in the bladder.

In attempting to account for the presence of the teeth in this situation, it has been supposed that they may have been swallowed and so have passed by ulceration from the intestinal canal to the bladder ; but this mode of accounting for the phenomenon, is the more difficult, from the absence of all antecedent symptoms of abdominal disease, and the occurrence having taken place in the female ; unless indeed we may suppose the communication with the bladder to have occurred far up in the intestinal canal. The generation of the teeth in a diseased ovary, and their transit thence to the bladder, is a supposition equally difficult to reconcile with the history of the case ; and we are left but one mode of accounting for the occurrence, by supposing that the teeth may have been introduced by the patient herself ; an explanation for which there is no sanction in any thing that could be gleaned from the history of the case ; but which certainly derives countenance from the records of some extraordinary cases of this description. I have not been myself, however, led to adopt this opinion, but rather incline to the idea of the teeth having found their way from the intestinal canal into the bladder ; though nothing in the history of the case enables me to account for their having done so. What was of most consequence to the patient, however, she was discharged from the hospital perfectly well, on the 12th of October ; after having been nine days under treatment.

ART. III.—*Medical Cases and Observations*. By CHARLES
WARBURTON RIGGS, Surgeon to the Mullaglass and Cam-
lough Dispensaries.

CONTRACTION OF THE CHEST, CONSEQUENT ON PLEURITIC INFLAM-
MATION.

SAMUEL WHITE, aged 27, became a patient at the Mullaglass dispensary on the 28th of March last, presenting the following symptoms:—much emaciation; expression of countenance anxious; breathing hurried, frequent slight cough, with trifling mucous expectoration; voice weak, and articulation abrupt. Pulse 110, small, but firm; skin hotter than natural; perspires freely on the breast and forehead towards morning; bowels pretty regular. Cannot lie on the right side; sleeps badly, and often awakes agitated by alarming dreams; appetite much impaired. Left side of thorax dull throughout on percussion; this was rendered peculiarly evident from the opposite side affording an unusually clear resonance; expansion of the affected side hardly perceptible, that of the other proportionably augmented. The stethoscopic signs were absence of the vesicular murmur, except over the subclavian, acromial and upper scapular regions, where it was heard feebly; that on the sound side very loud, much more so than I had previously heard it under any circumstances of age or disease. Heart's action visible at the apex, which occupies nearly the natural position, and its impulse audible over the greater extent of the diseased side. The account he gave of his illness was, that on the 12th of January last, when overheated and fatigued, he cooled suddenly, but experienced slight bad effects; after three days he again underwent much exertion, was overheated, and again allowed himself to cool incautiously. In the course of that night he was seized with acute lancinating pain under the left mamma; the stitch he describes as having been most

intense. Heated salt, which is usually resorted to by the common people in this part of the country under similar emergencies, was applied to the side, and in two days from the beginning of the more violent attack, he was almost as instantaneously relieved. The subsidence of the pain was immediately preceded by a sensation of something giving way within the chest. For two days prior to the urgent seizure, there was, he states, an indistinct pain, or rather uneasiness on either side of the lower part of the sternum, unattended by any considerable febrile disturbance. This feeling continued, in some degree, up to the period at which he came under my observation, and was that to which he referred me for the cause of his ill health. He was pertinacious in attributing the seat of this disease to that situation, and of ascribing all his other symptoms as originating in the cause of the uneasiness there. He had been treated for a gastric affection, had applied a blister to the epigastrium, and used other remedies without experiencing any benefit. The history and symptoms, general and local, were characteristic of effusion into the left pleura; and the following stethoscopic phenomenon, elicited by the effect of position and gravitation on the effused fluid appeared to furnish a positive confirmatory indication. On exploring the upper scapular region, immediately after his having reclined backwards, respiration was inaudible; but after the position forward had been maintained for some time a faint murmur was appreciable. I repeated the examination of this fact several times, with the same result from each trial. He was put on the use of blue pill in small doses, conjoined with squills, and had a blister applied to the side. There was an unequivocal amendment in a week; he could lie with ease on the unaffected side. Pulmonary irritation nearly gone, the cough and expectoration having almost ceased. Pulse 100; gums slightly tender. This treatment was continued with certain modifications, the fluid undergoing progressive absorption, which failed, however, to restore the sound on percussion, or that of respiration, and a

new feature, that of contraction of the chest, was observed to be developed, becoming marked in an exact ratio to the absorption. As there has been no variation in the condition of the chest for the last two months, I look upon the deformity as having reached the limits to which this rare and obscure form of disease extends, and presenting those distinctive characters of this peculiar sequence of pleurisy as prominently at present as can be expected at any subsequent period of the person's life. The diseased side is found, on admeasurement, to have shrunk three-fourths of an inch. On inspecting the motions of the chest, those of the left side have almost ceased. There is very slight, if any, elevation during inspiration, and consequently no depression accompanying expiration, whilst, as would be anticipated, on the healthy side these motions are increased: there is heaving resembling the breathing of an asthmatic person; the muscles of respiration seem, as it were, to aid the lung in the execution of its double function. In a state of quietude the inspirations amount to twenty-four in the minute; but the slightest effort, even that of speaking, produces marked acceleration. The posterior costa of the scapula projects from the flattening of the side, which withdraws the natural support from that bone, and which, for obvious reasons, occurs most anteriorly, or towards the inferior costa. Respiration is faintly audible under the clavicle, gradually diminishing to the intercostal space between the third and fourth ribs, where it ceases to be heard. In the upper part of the back it exists to about the same extent, and in the same degree. The sound in percussion presents similar gradations; the dulness increasing from above downwards.

I shall now offer a few observations on the pathology of this affection, and in these I will be chiefly guided by the researches and opinions contained in Laennec's work, referring the symptoms that were present at the invasion, and during the progress of the case under consideration, to the facts and principles deduced by that author, and attempting an analogical

inquiry to ascertain, by rational inferences, in what degree his conclusions will tend to illustrate the phenomena that were manifested in the foregoing case. On reviewing the symptoms, I think it may be inferred from the undefined uneasiness experienced at the lower part of the sternum, and the general indisposition, supervening on fatigue, and sudden cooling, that inflammation in a *latent* form existed previous to the invasion of the acute pain, and that the violent pain indicated an intense augmentation of the inflammatory action; and also that the instantaneous subsidence of the pain, and consequently the abatement of the high degree of inflammation may be attributed to sanguineous, or sero-sanguineous effusion; or, according to the pathological nomenclature of Laennec, *acute hæmorrhagic pleurisy* arose during an attack of *latent pleurisy*. The sudden cessation of pain, and the subsequent condition of the patient, authorize, I think, the opinion that an internal hæmorrhage occurred, which controlled the inflammation, as observation proves it may do. There exists a diversity of opinion as to the cause of the more complete forms of contraction of the chest. Laennec conceived that hæmorrhagic pleurisy could alone give origin to it, whilst other pathologists, doubting the propriety of limiting the production of the affection to a single form of disease, have assigned other states of disease within the thorax, as contributing to, or occasioning its formation. Laennec ascribes the production of the deformity to the transformation of the pleural envelope into a fibrous or cartilaginous membrane, retaining permanently the compressed lung; and, therefore, as the absorption advances, the chest recedes; and, finally, when the absorption is complete, and the costal and pulmonic surfaces are in apposition, or rather their adventitious membranes are approximated, they become, by an ulterior process, agglutinated. I think it will appear pretty evident, that in this case, from the time of the attack till he was put under treatment for the affection, very slight, if any,

16. Mr. Riggs' *Medical Cases and Observations*.

absorption of the effusion had taken place ; and during this, a period of more than two months, the lung was compressed in a very considerable degree. Whether the non-expansion of the lung was owing to the duration of the compression, or its having an adventitious inelastic investment, binding it down, and preventing its expansion, must remain undecided. Perhaps both causes conspired to produce the effect, and that either singly would be insufficient to account for its production. This conclusion is perhaps justified by considering that it may not be invariably the result of hæmorrhagic pleurisy, and the consequent formation of the fibrous, or fibro-cartilaginous covering, but may arise from other morbid conditions, the exact characters of which remain to be determined by pathological inquirers. A subject with this deformity was brought for dissection to the anatomical rooms in Trinity College some years ago ; I was then a student at that school, and shall never cease to regret the opportunity I then lost of having this morbid condition inspected. The body was that of a lad about puberty ; on the left side of the chest, the lung was shrivelled to the size of a very small clenched hand, invested by a thin, but compact membrane, of a bluish white colour, glistening, and bearing a close similitude to the tunica albuginea of the testis. It did not adhere at any point to the costal surface. There was no fluid in the chest, and the degeneration seemed to have obtained for a considerable period. Here there must have co-existed with obliteration of the lung, considerable contraction of the thoracic parietes ; *and yet one anatomical character, which Laennec has given, the adherence of the pulmonary to the costal surface, was absent.* This I can state with the utmost certainty, as I was engaged in dissecting the body, and was struck with the peculiar smooth and shining appearance the surface of the contracted lung presented.

SINGULAR EFFECT FROM THE APPLICATION OF TARTARIZED
ANTIMONY.

In a paper treating of thoracic disease, it will not, I conceive, be inappropriate to subjoin the particulars of a singular growth, resulting from the application of tartarized antimony. I have not seen any similar instance noticed ; nor has it, so far as I could ascertain, been observed in Dublin or Edinburgh. I am, therefore, probably right in applying the epithet singular to this production. It occurred on the breast of a man about twenty years of age ; and in its dimensions and figure bore a pretty close resemblance to the expanded hand of a labourer : it was of uniform thickness, from eight to ten lines, very irregular on its circumference, occupying the centre of the sternum, and extending laterally over the sternal portions of the cartilages of the ribs, with which it seemed firmly incorporated, being altogether immoveable. The surface of the tumour had a shining hue, and a constricted tense aspect, very like the skin on a cicatrix of some months' duration. Minute, tortuous, but very distinct vessels were seen through the integuments covering it. It was dense, firm, and unyielding, and possessed all the physical characters of cartilage. It was devoid of sensation, and the compression, or rubbing of the fingers, gave no pain, nor any feeling but that imparted to the skin. The man said it was intensely itchy when his surface was heated ; but that, and the annoyance from the bulk, were the only inconveniences it occasioned. At the time at which this description was taken it had existed nine months. Mr. Wilson, a distinguished surgeon, who has since retired from professional life, saw it at the same period. The tumour was produced by the hasty and indiscriminate imunction of tartar emetic ointment, the circumstances of which were thus related by the man :—he had obtained a box of the ointment from a young man in the neighbourhood, a phthisical

patient, with directions to apply it night and morning, as he had himself done until the pustular inflammation was excited, to remove a catarrhal affection, with a high encomium, and promise of immediate efficacy. Instead of conforming to the directions, he rubbed the ointment in after very short periods, as that of one or two hours, until the eruption appeared, and then continued the application indiscriminately over the inflamed and suppurating pustules, until an intense general inflammation arose, the pain and tumefaction of which forced him to desist. No means were used to allay this violent action; extensive suppuration and separation of the part ensued, followed by the gradual generation of the growth described. The nature and cause of this formation appear simple and explicable, by attending to the physiological fact, that however identical the primary effect of inflammation may be in the different tissues, and on the various surfaces in the body, there is a tendency to transformation or assimilation of the adventitious formation to a structure similar to that by which it was furnished. This is exemplified, for instance, in inflammation of the serous, mucous, and cellular membranes, each of which, under inflammation, secretes fibrine, which finally assumes the organization of the tissue that produced it. The result of inflammation of the periosteum, as observable in cases of one species of node, and the state of the texture that exists in the progress of exostosis, so ably described by Professor Macartney, may be adduced as bearing on this principle, and as serving especially to explain the subject under consideration. It may, I think, be assumed, that the tumour was occasioned by the inflammation having been communicated to the perichondrium and periosteum, giving rise to a gelatinous exudation, which gradually increasing, and acquiring a concrete form, ultimately attained the cartilaginous consistence.

ART. IV.—*Report of a Case in which a Foreign Body was supposed to be present in the Trachea.* By RICHARD T. EVANSON, M. D., Lecturer on the Materia Medica at the School of Anatomy, Medicine, and Surgery, Park-street, and one of the Medical Attendants at the Institution for Diseases of Children.

(Read before the Surgical Society of Ireland.)

IN the month of August, 1832, Mary Brown, an infant, one year and one month old, was brought to the Institution for the Diseases of Children. The countenance expressed great uneasiness. The face was pale and swollen ; and when I first saw the child, it had one of the hands applied to the throat, as if to indicate the seat of distress. On a nearer approach I was struck by the peculiarity of the breathing ; respiration was difficult, and a stridulous sound on inspiration, suggested the idea of croup ; but this idea was at once abandoned, being supported neither by the peculiarities of the symptoms, nor the history of the case.

On closer examination, the child was found to be hot, restless, and uneasy, and the pulse rapid. Expiration was short, but not impeded, nor accompanied by any peculiar sound. Inspiration, on the contrary, was long, forced, and difficult, and accompanied by a rough, rather stridulous sound. The child was hoarse ; and on looking into the mouth, the tonsils were found enlarged, and the back of the pharynx inflamed. No fit of coughing occurred during the examination ; but the mother stated that the child was subject to severe fits of coughing, which, however, from her description, did not seem to resemble the cough of croup. The throat appeared swelled externally, more so than the face ; and the hand continued often to be applied to the trachea as if uneasiness were felt there. The cry, though hoarse, became clear when the child screamed aloud. Bronchitis of the right lung was indicated on applying the stethoscope.

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It was evident from the symptoms that the trachea was the seat of disease, but it was not a case of croup : what then was it? To the history of the case we were to look to determine the diagnosis. I suspected the presence of a foreign body in the trachea, and inquired if the child could have swallowed any thing likely to stick in its throat ; when it appeared that up to Friday previous to the day (Tuesday) on which the child was presented at the institution, it had enjoyed perfect health. While its parents were at dinner on Friday, the child, who was present, caught hold of some herring, which it forced into its mouth. Immediately, it was seized with a violent fit of coughing, and threw out what it had attempted to swallow, pointing to the ground where it fell. This was carefully examined, but no bone could be detected. The child continued to cough violently, and the mother tried to force down with her finger whatever might be sticking in the throat. On withdrawing her finger, the child appeared to become hoarse. To this violence may be attributed the inflammation of the tonsils and pharynx.

The child passed a sleepless night, coughing and hoarse. On the next morning, the peculiar stridulous breathing (already noticed) was perceived, from which we may infer it to have been the consequence of inflammatory action set up during the night. Castor oil was administered by the mother, we need hardly add, without any benefit to the symptoms, which becoming each day more severe and constant, the child was brought to the Institution on the Tuesday following ; the fourth day after the accident, previous to which the child had been in perfectly good health.

After hearing the history just detailed, little doubt was left on my mind, that the cause of the child's sufferings was the presence of a foreign body in the trachea ; most probably a bone of the fish which the child had attempted to swallow. The particular character of such a foreign body, sharp and adherent, and calculated to cause inflammation in the mucous membrane of the trachea, would account for the peculiarity of

some of the symptoms, differing from those more commonly accompanying foreign bodies in the windpipe, especially such as are smooth and moveable.

The usual stethoscopic signs of alternate freedom or obstruction to respiration were absent; and a sound subsequently heard, as if indicating the movement of a smooth body up and down the trachea, was found to depend on the passage of bubbles of air, caused by respiration in the mucous secretion, with which the trachea became ultimately loaded. The constancy of the stridulous sound in respiration indicated the inflamed state of the mucous membrane, which could not fail to have been induced by the injury consequent on the presence of a sharp and wounding body, and this view was confirmed by the fact of this peculiar sound not having been heard until the morning after the accident, when there had been time for the occurrence of inflammation. This sound, though resembling that of respiration in croup, yet formed the only point of resemblance; the cry being quite clear when the child screamed aloud; and the character of the cough being materially different; neither indeed did the cough resemble that commonly attendant on the presence of a foreign body, it did not take place in sudden and violent paroxysms, with comparatively complete tranquillity between, but was more of a harassing, teasing kind, though occasionally in a severe fit enough. The peculiarity moreover of the difficulty of breathing taking place during inspiration, and not expiration, made a difference between the symptoms of the present case and those usually observed; especially when the foreign body is round and smooth; but the foreign body in this case (if any) was sharp, angular, and likely to remain fixed; while the history of the case left little doubt that a foreign body had been swallowed, and what that foreign body was.

After this statement, the case may appear clear enough, but when first presented, and before the history and symptoms could be thus collected and collated, it will be conceded that

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the case was one of some difficulty, if not doubt. Under these circumstances, and feeling that operation would most probably become necessary, I had the child brought to Mr. Crampton, consulting surgeon to the Institution, who examined the case with much care. He passed a bougie to remove the possibility of any substance adhering in the œsophagus; when strangely enough, the child was found to experience immediate relief, the breathing having become freer and more easy. This amendment, however, was but temporary, the former symptoms quickly recurring. As the child did not appear in any immediate danger, and some of the symptoms at least, were to be attributed to inflammation, it was determined to try the effect of active antiphlogistic treatment, before recourse was had to operation, especially as the mother evinced a determined opposition to such a proceeding. Leeches were ordered to be applied to the throat; and a purgative of calomel and jalap, with a strong solution of tartar emetic, prescribed. Next day the child was presented at the Institution, but no relief seemed to have been experienced from the remedies employed. The leeches had bled freely, the child had been well purged and often vomited, but the symptoms appeared to have increased rather than to have been mitigated, and even the bronchitis was found to have extended; and now occupied both lungs. The child was again seen by Mr. Crampton, and operation immediately decided upon, but some hours elapsed before the parents' consent could be obtained, or they got to bring the child to the Meath hospital, where Mr. Crampton was to have it under his care, as at the Institution for children accommodation for intern patients is not provided. The child was now seen by several medical men, all of whom agreed in the diagnosis already given, there being no second opinion as to the nature of the case.

When brought to the hospital the child was found to be in urgent danger, all the symptoms had become rapidly aggravated, and the general strength appeared failing fast. The surface

was cold, face somewhat livid, and eyes glassy ; no cough was heard, but much uneasiness was apparent.

It is unnecessary to detail the steps of the operation which was performed by Mr. Crampton ; suffice it to observe, that a little time was permitted to elapse after the external incision had been made, to allow of the subsidence of the hemorrhage ; a useful precaution worthy of notice. On plunging the knife into the trachea the child screamed aloud ; but after the incision was completed, the voice was lost. The child started convulsively (the mouth being covered with foam) and appeared in imminent danger of suffocation ; but on the excision of the piece of trachea being completed, relief was obtained. The bone was carefully searched for, but could not be detected, though an elastic bougie was passed upwards two or three times, through the wound. The child was now restored to its mother, and placed in bed ; no immediate or very palpable improvement having attended on the operation, such as often occurs in cases where foreign bodies in the trachea have been operated for and removed ; but which we are not always immediately to expect, even when the body is moveable in the passage.

On the morning after the operation*, however, some amendment was manifest ; though the child still continued to suffer much from some of the prominent symptoms. The respiration was laborious, stridulous, and wheezing, being seventy in a minute, while an occasional violent fit of coughing was necessary to remove the copious secretion of thick mucus that blocked up the opening in the trachea, which was with difficulty kept clear. The child, however, began progressively to improve, calomel and hippo being the only medicines administered. An occasional fit of violent coughing, at times threatened suffo-

Mr. John Hamilton kept an accurate daily report of the progress of the case, while in hospital, with which he has kindly furnished me.

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cation, but was to be attributed to the cause already assigned—the blocking up of the opening, with the profuse and tenacious mucous discharge.

On the Monday following the operation, the quantity of this discharge was observed to be sensibly diminished: the cough was slighter and less frequent; respiration being better performed through the natural passage.

The child continued to improve, and the wound, which had much contracted, now allowed of being closed without inconvenience: but the cough was still occasionally very troublesome, and assumed so much of the character of pertussis, that a mixture containing carbonate of soda and opium was prescribed.

The mother now removed the child from the hospital, being provided with this medicine. In a few days she returned, bringing with her the child, which had now nearly quite recovered. The wound had completely healed, and the natural respiration been restored; but some cough still remained, and the voice continued faint and hoarse, as was indicated when the child made a noise or cried. The change of voice continued for some time, nor was it restored to its natural sound for several months. But it has since become quite natural, clear, full, and loud; while the cicatrix from the wound has contracted so much, as to leave little or no deformity, being, in fact, hardly perceptible.

The peculiar character which the cough had latterly assumed, with the continued hoarseness and feebleness of voice, may be, perhaps, accounted for by the irritation of the glottis, naturally connected with the inflammatory action going on in the upper part of the larynx; some cases being on record, in which the adhesion of small foreign bodies (as saw dust) to the glottis, produced cough, very like pertussis; while the thickening, consequent on the inflammation, would account for the change of voice, and the resolution of that thickening, for its restoration.

This case cannot but be regarded as interesting and impor-

tant. That the child had been rescued from impending death, by the operation of bronchotomy, no doubt could be entertained, and this may be deemed a sufficient corroboration of the correctness of the opinion which led to the performance of that operation ; for had a different course been adopted, it is but reasonable to suppose that a different result would have taken place. Still it was unsatisfactory that no foreign body had been found, and that neither during the operation, nor at any time subsequently, was the bone detected, though anxiously looked for by the medical attendants. Undoubtedly we may suppose that the bone, though present, might have easily escaped amid the quantity of mucus that was expelled both by the mouth and through the artificial opening, and so have eluded detection. But still it was dissatisfactory that we should want this unequivocal proof of the correctness of our practice, though this by no means impeached the fact, that by that practice the child was saved.

But even this source of dissatisfaction is removed, if we may rely on the statement made by the child's mother, and which relieves the history of the case from all imperfection ;—if, indeed, it is to be relied upon : for this, however, I cannot vouch ; but I feel called on to state what I have been told.

On going some days after the child's removal from the hospital, to inquire after it, I stated my surprise that no bone had been found, when I observed a look pass between the parents, which made me suspect some concealment, and I immediately charged the mother with deception. She then confessed that she had herself extracted the bone, but had concealed it ; at first, through fear that farther operation might have been resorted to, and subsequently through dread of censure, for having herself meddled, and then concealed what she had done. She produced a portion of fish bone, which she had carefully wrapped up in paper, and which she stated to be but part of what she had originally found ; another portion broke off and was lost while I was examining it, so that the

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fragment which I was able to preserve, is but a small part of the bone originally found. This appears to be part of the bone of a herring, or other small fish, and was of its usual colour, though somewhat less firm than natural, when I obtained it. The woman's story is this ; on the fourth night, after operation, she thought she felt something hard stick in the sponge with which she was clearing away the mucus from the wound. A projecting point now began to appear whenever the child coughed, and the direction of this body seemed to be from the upper part and side of the wound. She assigns, as her reason for not informing some of the medical attendants of this fact, her dread that another operation would be resorted to for the extraction of this body. It projected so much before morning, that she was induced, herself, to attempt its removal, which she accordingly effected, but not without being obliged to use some force, and a small quantity of bloody matter followed, with which, also, the extremity of the bone was stained—this extremity, which she pointed out, being sharp and hooked. When removed, she states the bone to have been soft, and of a greenish colour, like that of the mucus expectorated. On putting it into some whey to steep, this colour disappeared, and the bone lost its softness on drying. She says that she intended to have told me this when she saw me, which did not occur until my visit to her house, and then she made the statements I have mentioned. One circumstance she distinctly dwelt upon, that the child was found better next day, and breathed more freely through the mouth. This statement I find corroborated according to date in Mr. Hamilton's report, who, at the time, knew nothing of the woman's story, and does not, I believe, now credit it, though he expresses no doubt that a foreign body had been present in the trachea. I do not enter into any discussion as to the probability of the woman's statement ; I rather incline myself to believe it : but though its veracity would render more satisfactory the result of the case, as leaving nothing to conjecture, it is by no means essential to establish the correctness of the diagnosis or practice.

A striking and unexpected corroboration of the correctness of both, is derived from the details of the following case, most curiously similar in its history and symptoms, though far different in its result ; for the particulars of which I am indebted to my friend Dr. Maunsel.

A healthy child, about two years old, was suddenly seized with a paroxysm of coughing, followed by stridulous breathing, and so much dyspnœa, as appeared to threaten instant suffocation. The case was treated by a medical man as one of croup, which, in fact, it closely resembled. Relief appeared to follow the treatment, but similar paroxysms recurred from day to day, and became on each repetition more alarming. About ten days after the first attack, Dr. M. saw the child in the absence of the medical attendant. It was much debilitated ; the breathing remained permanently stridulous, and paroxysms threatening suffocation, followed upon the least excitement. As the protraction of the disease threw doubt upon the supposition originally entertained of its nature, a more accurate inquiry was instituted respecting the circumstances attending its commencement ; when it was discovered, that at the moment of the first seizure, the child had been sitting upon the knee of one of the servants, while the latter was dining upon fish. From a consideration of these circumstances, Dr. M. imagined that the symptoms might probably be accounted for by the presence of a portion of fish bone in the trachea, and suggested the performance of bronchotomy. As the case was enveloped in a good deal of obscurity, the operation was not, at first, acceded to. Eventually, however, it was performed, about three weeks after the accident, but the child was so much weakened, that it expired immediately after the operation. A portion of herring bone was found lodged in the ventricle of the larynx.

From the history and result of this case, we derive a most instructive corroboration of the justness of the views entertained respecting the preceding one, and almost cease to feel any concern as to whether the statement of the bone having been discovered in that instance be true or not. At all events, the history

of these cases establishes, that the occurrence of a foreign body in the trachea of an infant, leading to symptoms closely simulating those of croup, and capable of being confounded with it, is not an isolated fact. We have hence a salutary warning (if such indeed be wanting) of the necessity for the most scrupulous exactness in investigating cases of disease occurring in infants, who are themselves unable to afford us any clue as to the nature of their sufferings, while those around them are but too often interested in deceiving and misleading us, in order to conceal their own neglect or mismanagement.

When called upon, then, to treat a case of croup, (for every affection of the throat, accompanied by any wheezing sound, is so called by the attendants of an infant,) we may bear in mind the possibility of a cause for the symptoms present, far different from what may have been suspected, and the consequent necessity for timely recourse to operation.

Far be it from me to wish to inculcate or induce a hasty recourse to such a proceeding, or to represent bronchotomy in the infant, as a trifling or even easy operation, such as it appears to be when performed on the still, attenuated subject. No one who has witnessed its performance in the living infant, beset as it is with difficulties and dangers;—the short neck, numerous blood-vessels, narrow, deep-seated windpipe, in frequent or convulsive motion,—can for a moment think lightly of this operation, or feel inclined, heedlessly, to undertake it. Yet we have just seen two cases, in which its performance became unexpectedly necessary, and in one of which, its timely aid seemed to avoid impending dissolution; while in the other, its undue procrastination hastened a death, which its earlier employment might, most probably, have prevented.

From these cases, also, we may glean a few points of general practical interest. We see that the irritation of a sharp foreign body in the trachea of a child, leads to an inflammation of its mucous membrane, ultimately extending over the whole bronchial tubes, accompanied by copious mucous or purulent secretion, and attended by stridulous breathing, much re-

sembling croup; but not (it would appear) leading to the formation of the false membrane, so peculiarly characteristic of that disease. Had the foreign body been even coughed up, the resulting inflammation might still render the case subject for operation, like those instances in which croupy symptoms are induced by the inflammation of the larynx or upper part of the trachea, consequent on the irritation caused by the swallowing of vitriolic acid, or boiling water: and it is in such cases, before the inflammation has extended downwards, that bronchotomy is so serviceable, by affording the means of respiration, until the inflammation that obstructed the natural passage has subsided, or been subdued. Under such circumstances, relief is afforded by timely opening of the trachea, even though the foreign body, when such is the cause, be not removed, but this we may at least suppose capable of escaping or being removed from the opening several days after it has been made.

Whether such a result is to be looked for, or may be attempted to be brought about by instruments, when the foreign body is situated lower down in one of the bronchi, is a question of great importance, but of ulterior research, which I do not now venture to enter upon. It has already received attention from Dr. Brown, in an elaborate essay published by him in the *Edinburgh Medical and Surgical Journal*; and I doubt not will derive farther illustration from the pen of Dr. Houston, in the very curious and interesting case, contained in this number.

ART. VI.—*Two Cases of Popliteal Aneurism.* By MAURICE COLLIS, A. M., one of the Surgeons to the Meath Hospital.

CASE I.—*Double Popliteal Aneurism, of fifteen years' standing. The right Aneurism becoming diffuse from injury. Ligature of both Femoral Arteries. Recovery.*

November 1, 1833. James Brady, æt. 38 years, a strong, healthy man; formerly an infantry soldier, and consequently

exposed at times to violent and fatiguing exercise. In the year 1818 (without having received any injury of which he was aware) he *felt a pain in both hams, and soon after perceived a tumour in each popliteal space.* These became somewhat larger when exposed to fatigue or cold; from the commencement a throbbing was felt in the tumours, but the patient was never prevented from attending to the duties of his occupation. These tumours continued stationary until August, 1833, when his right leg was much and severely bruised between two cows, (he was at this time employed as a cattle driver). The tumour, in this limb, shortly after increased and became painful; the throbbing augmented. The leg and thigh became swollen, and the foot numb and cold.

He came to the Meath Hospital in this state about six weeks ago, and as he refused to remain in the house at that time, I directed a cold evaporating lotion to be kept on for some time, and subsequently a moderately tight roller; under this plan the swelling diminished and the pain abated. He shortly after resumed his labours, and exercised his limbs considerably, and in consequence, the tumour began again to increase. His leg and foot became swollen and œdematous, and he complained of numbness, and a want of sensibility in the limb. The tumour in the ham became more diffused; and the pain, and an uneasy sense of constriction in the limb deprived him totally of rest. At this period there was no alteration in the aneurism of the left limb. In this state he was admitted into the hospital; the veins upon the surface of the limb were very distinct, and the saphena particularly prominent. There was a slight erysipelatous blush over the inside of the knee. Pulsation much diffused over the tumour. Pulse 90, rather hard; tongue clean; bowels regular. His general health was but little affected. There was not the slightest evidence of other disease in the remainder of the arterial system. He never suffered from palpitation of the heart, or difficulty of

breathing. He was placed in bed, kept quiet, allowed moderate diet ; some purgatives, and an opiate at bed-time, and a cooling evaporating lotion to the swelling. The pain diminished, and the external inflammation subsided ; but the tumour became daily more tense and extended : so that upon consultation it was determined to tie the right femoral artery, as the limb, as well as his life, became more and more endangered. I performed the operation on the morning of the 10th. The vessel was tied in the upper third of the thigh, with a single round ligature, consisting of three threads of three-corded silk. The patient did not lose a spoonful of blood in the operation. This case proceeded favourably, and the ligature separated upon the seventeenth day. From the period of its application the tumour gradually diminished, and the swelling of the leg entirely disappeared.

December 3. Able to walk about a little. The surface of the tumour is soft, and gives a slight sensation of fluctuation. It will, in all probability, suppurate before the patient gets quite well.

On the 14th, I tied the left femoral. In endeavouring to separate the artery from the vein, I made a small opening, about the size of a pin's point, into the artery, from which a rapid jet of blood took place. The artery was immediately compressed against the pubis, and a double ligature applied ; one part of which was tied upon the highest exposed position of the vessel, and the other upon the lowest, leaving between both about a quarter of an inch of the artery. He did not lose more than about an ounce or two of blood. The tumour disappeared almost entirely after the application of the ligatures. The limb retained a moderate degree of temperature ; every thing went on favourably, and on the 27th both ligatures came away. This man's health is daily improving. His appetite is good, bowels regular. He walks about every day, and is gaining strength. The tumour in the left ham has almost entirely disappeared ; that in the right is daily becoming smaller.

This case presents many points of deep interest ; among these, the following seem to me to be the most remarkable :

1st. The great length of duration of the aneurismal tumours without their being productive of any unpleasant effects ; they existed almost stationary from the year 1818 to 1833. I am not aware of any similar case being upon record.

2d. Both tumours occurring simultaneously, a very unusual circumstance, and the more particularly so, that there does not appear to have been any general disease of the arterial system.

3d. The disease occurring in *both* situations without any sensible injury.

4th. The little inconvenience suffered by the man ; he was never prevented from attending to his duty, which at times must have been very laborious, until the period of his first application to the Meath Hospital.

In this case we had to consider many circumstances before undertaking the first operation. It was true, that the disease existed in more places than one ; but the conditions under which the patient had so long borne the disease were changed ; the right aneurism had become diffused from injury. The limb œdematous, and inflammation setting in. No alternative remained, but to tie the vessel, or allow the man to die of gangrene.

This operation having succeeded, I was encouraged to the performance of the other by the general healthy state of the arterial system, as shewn by the great length of time which elapsed from the first period of disease, by the patient's bearing the first operation so well, and from the importance of placing him out of the danger of an accident, such as had happened to the right aneurismal tumour.

CASE II.—*Popliteal Aneurism in a healthy subject. Operation. Copious hæmorrhage from the wound on the eighth and thirteenth days. Employment of pressure. Recovery.*

November 8, 1832. Michael Maddock, æt. 30 years ; a

labourer, generally very healthy ; resides in the country, about five miles from Dublin ; applied for relief this day. He has a large pulsating tumour in the left ham, which completely fills up the popliteal space, and which projects very much toward the outside. The swelling is hard, smooth, and elastic ; pulsation strong ; the *bruit de soufflet* very distinct ; a slight blush of redness over the most prominent part of the swelling. By compressing the femoral artery (the action of which is very great) all pulsation in the tumour ceases. He cannot bring his leg into a straight position. The leg is swollen and œdematous, and of a mottled colour. Complains of much pain in the ham, and numbness of the leg and foot. His pulse is about 100 ; no fever ; no indication of disease of the heart, or arterial system generally.

About five months ago he first felt pain in the ham, but did not perceive any swelling until three months subsequently. He continued at his work until three weeks since, when the tumour became suddenly larger, and very painful ; the limb became much more œdematous, and he was obliged to remain in bed. A few leeches and a cold evaporating lotion were applied with relief.

OPERATION.—12th. I tied the femoral artery in the upper third of the thigh, at the usual place. Nothing particular occurred during the operation, excepting a little delay arising from some difficulty which was experienced in passing the needle around the artery, in consequence of its peculiarly close adhesion to the vein and sheath. Upon the application of the ligature the pulsation in the tumour immediately ceased, and he was placed in bed, the wound being dressed as usual. In the evening he complained of shooting pain along the back of his leg and thigh ; did not sleep. No alteration in the size of the tumour, the apex of which is of a light purple or copper colour.

15th. Suffered much from pain in the foot and back of the leg. The muscles of the entire limb were occasionally thrown

into spasmodic action. A cold evaporating lotion was applied to the ham, which gave considerable relief. The leg and foot œdematous; the tumour feels more firm and contracted.

20th. Scarcely any alteration since the last report until this day. Upon dressing the wound, he complained that it felt much hotter than before, and a slight streak of blood was observable upon the dressings. About four o'clock, P. M. he experienced a sensation as if something gave way suddenly, and immediately afterwards he observed blood to gush from the sides of the wound. Before assistance could be obtained he lost, it was calculated, about twenty-four ounces of blood. Temporary pressure with lint and the hand was applied, and I was summoned to his assistance. I found the man's bed loaded with blood: I made an assistant compress the artery as it passes over the pubis; I then opened the wound through its entire length with my finger, removed all the coagula; I then applied a small firm compress to the bottom of the wound, which I filled up with pieces of sponge, and over these I applied a *presse artere*, with a graduated screw, which was kept very tight until the 22d, when it was loosened, but not removed. During this interval he was bled from the arm, purged, took digitalis, &c.

25th. Some degree of erysipelatous inflammation having been produced upon the upper part of the thigh, and anterior part of the abdomen, from the pressure caused by the instrument, it was removed.

27th. On this morning I took away all the sponges and lint, which were soaked in purulent matter, and came away without any difficulty. The wound looks healthy, and is granulating.

December 1. A small abscess formed on the upper and outer part of the thigh where the instrument pressed.

3d. About three o'clock this day (being thirteen days from the first attack of hemorrhage) a second took place; he lost only a few ounces of blood, as it was immediately arrested

by our intelligent apothecary, Mr. Parr, who instantly filled the wound with sponge, and applied the screw, which was left on until the 7th, when it and the sponges were removed. During this period the state of the patient seemed nearly hopeless. His countenance was almost hippocratic; he resembled a person in a low stage of typhus, and a gangrenous smell exhaled from the body. He was freely supported by wine, bark, and animal food. He had no further bleeding, and began to recover rapidly. On the 4th of January he was sent home, and soon after resumed his labours.

The instrument which I made use of was one previously employed by Mr. Crampton, the Surgeon General, in a similar case, and with good success. It consists of an iron hoop, about an inch and a half in breadth, and sufficiently large to encompass the thigh; it opens behind, so as to admit of such dilatation as to suit a limb of any dimensions. This hoop has a slit in the anterior and internal part, into which is placed a moveable screw, with a pad attached. This pad is to be applied immediately over the compress, which has been put upon the artery, and then by turning this screw, any degree of compression can be made upon the artery, whilst little or no pressure is made upon the other parts of the limb, and consequently no great obstruction, if any, is given to the collateral circulation. The hoop produced much inconvenience to this man, and was also the cause of inflammation, which terminated in abscess. This arose from the instrument being too narrow upon the part on which the limb rested, and I have now got one constructed with a broad flat surface; this is padded, and fits the under and outer part of the limb accurately, and on which the thigh rests without any inconvenience. To this are added two straps, one to surround the pelvis, and another the thigh, by which means the instrument is kept more firmly in its situation.*

* This instrument was made by Reynolds, of the Coombe, who has shown much intelligence in constructing this and other instruments.

My chief object in recording this case is to show the advantage to be derived from pressure properly made, in arresting secondary hemorrhage, from such a vessel as the femoral artery, and the greater benefit likely to arise from such a mode of proceeding, than from tying the artery higher up, which, in the majority of the cases in which it has been tried, has failed in saving the life of the patient.

ART. VII.—*Researches on Solidification of the Lungs in New-born Infants.* By EDWARD JOERG, P. D., C. M. B. Leipsig.

THE greater number of new-born infants die of convulsions, apoplexy, or suffocative catarrh; so, at least, we are told by the parents, and even by the medical attendants, who are often unable to give us any clearer account of the matter, or unwilling to take the trouble of finding out a more accurate explanation of it. And yet the sudden death, or remarkable wasting away of so many children, should incite every observer to do his utmost to investigate the hidden causes of such melancholy occurrences. According to some experiments and observations made in the Leipsic School of Midwifery,* the various conditions and accidents which so frequently endanger or destroy the life of new-born infants, depend on one and the same organic disease of the lungs, resulting from either too difficult, or too rapid and easy a delivery.

It appears that the infant, immediately after its expulsion from the uterus, has to make a strong and deep inspiration, in order at once to commence and to continue effectually the new vital process connected with the great change in the circulation which then takes place. Nor has nature neglected to make

* *Dissertatio inauguralis pro summis in medicina et chirurgia honoribus cape-ssendis: "De morbo Pulmonum Organico, ex Respiratione Neonatorum Imperfecta orto;" auctore Edvardo D. Joerg, AA. LL. M. Philos. Doctore, et Med. Baccal. Lipsiæ, 1832, sumtibus Barthii.*

every requisite provision for this; since by the diminished vitality of the membranes towards the close of pregnancy, and the gradual compression of the placenta during parturition, the former circulation of the foetus is obstructed, and the scarcely born infant compelled to compensate by inspiring, for the deficiency of oxygen thereby produced. The first inspirations are, in consequence of this deficiency, (a state resembling asphyxia by carbonic acid or other poisonous gases,) naturally the most powerful, and at once expand the lungs to admit the rush of blood; so that the foramen ovale and ductus arteriosus may close and become obliterated: in which case the child continues to respire regularly, oxygenizes his blood, and is able to cry and to drink.

Thus the first cry of the infant, while it indicates its anxious, suffocating condition, serves at the same time to open and expand its lungs, and is consequently more analogous to the deep sighing and groaning of persons taken out of cellars or caves containing irrespirable air, than to a real expression of pain. Now, when any noxious influence prevents the development of this natural condition of the lungs, and produces the opposite one, it is evident that the consequences must be highly injurious.

Two cases have been observed, in which the respiration of new-born infants is rendered irregular, and organic disease of the lungs produced; namely, 1st, too difficult, and 2d, too rapid and easy a delivery. The former by too great compression of the head, and consequently of the brain, (whether in the pelvis during violent pains, or by the forceps,) weakens the infant to such a degree, that it is absolutely unable to relieve itself, and can only respire imperfectly, so that its lungs are but partially distended. The condition thus produced, is precisely similar to that to be presently described, except that in the latter, the pressure on the brain frequently produces inflammation of that organ, and thereby renders the case more complicated.

2. Too rapid and easy a delivery, in consequence of its short duration, and the inferior degree of compression of the placenta, does not produce the requisite obstruction to the foetal circulation, nor the due want of oxygen which makes respiration necessary. The consequence is, that the infant only respire imperfectly, expands and fills its lungs with air but partially, and is never able subsequently to remedy this without the aid of art.

Under the circumstances above mentioned, we have often seen infants suddenly seized with illness, and sometimes die in spite of every exertion made to save them, before the real cause of the attack and the proper method of treatment were discovered; and on examination the following appearances were observed, arising all from the same causes, though differing greatly among themselves in many respects.

In every case in which we made a post mortem examination for several years past, a portion only of the lungs, from the greater half to merely an eighth or tenth part, was found filled with air, and of a red colour; while the remaining portion continued in the same state in which it had been in the foetus, and was of a liver colour. When the infant had died soon after birth, the condensed portion was susceptible of inflation; but where death did not occur till several weeks after that event, it was found carnified and incapable of being inflated; sometimes the partition between the healthy and diseased portion was in a state of inflammation, and the latter contained vomicae: the bronchi, too, were often inflamed and filled with mucus. The great contrast between the bright red of the healthy, and the liver brown of the diseased portions, struck the eye immediately on opening the thorax. In most cases, the foramen ovale was still open, and there were very firm polypi in the heart and large vessels. The brain was frequently gorged with blood, which was sometimes even effused between its membranes and over its surface: it also occasionally contained abscesses corresponding to others on the cranium, or fontanelle,

that had been produced by the use of instruments, or by violent pressure against the pelvis during delivery. In the rest of the body, there was no particular morbid phenomenon constantly present: however, in the greater number of cases, the skin, particularly on the face, had a bluish cast; while in some it was withered and emaciated, and the whole body, especially the intestines, pale and bloodless.

From these facts, and from observations made of late years during the progress of the disease, we are warranted in describing its nature and terminations in the following manner: The solidification, or continuation in the foetal condition of a greater or less portion of the lungs, so that during inspiration their substance cannot be penetrated by the air. The blood, being still more incapable of penetrating, cannot be supplied with oxygen, and must consequently continue venous, and produce obstructions and dangerous congestions; while at the same time, from its being unable to afford the stimulus requisite to the system for the continuation of its functions, an atonic senile condition obtains, attended with the utmost weakness, and complete atrophy, and terminating in death in hectic fever. The general morbid condition is, consequently, difficulty of respiration and impeded circulation, producing dangerous and even fatal congestions. Its terminations are: 1st, recovery; 2d, secondary diseases; and 3d, death.

I. *Recovery* ensues when the efforts of the infant to inspire are assisted by proper treatment, and the subsequent symptoms properly managed.

II. *Secondary diseases*:—(a) obstruction of the lungs, inasmuch as a portion of them remains condensed, which, without actually producing death, is very oppressive and dangerous: (b) chronic cyanosis, the foramen ovale continuing open, and the infant being liable to constant suffering.

III. *Death*:—(a) from apoplexy; in consequence of obstruction and congestion: (b) from suffocative catarrh, when the feeble respiration is not able to expel the mucus secreted in

the bronchi, and the violent efforts at full inspiration produce bronchitis, and an over-abundant secretion of mucus, which the patient has not strength to get rid of: (c) from fever, the result of bronchitis: (d) from atrophy; the production of animal heat being prevented by the deficiency of oxygen, and the whole system paralyzed by the want of its requisite stimulus.

SYMPTOMS.—When the infant comes into the world, the head is either found greatly swollen, (in which case abscesses often form in the part that has suffered from pressure, and inflammation or violent congestion of the brain ensues,) or else, though quite uninjured, and the delivery having been rapid and easy, it cries but feebly, breathes very short, and exerts the muscles of the thorax greatly; it is presently attacked with a faintness, and if it had been capable of drinking previously, now loses that power, the voice becomes hoarse and weak, and scarcely audible. Stertor and convulsions soon follow, the little patient becomes quite blue, the eye-balls turn, and the respiration remits, sometimes for so long as five minutes, till the scene at last closes with death. Should the illness continue for some days or weeks, a little short cough, the most certain sign of violent bronchitis, comes on; together with total weakness, atrophy, and hectic fever; and the child, at the very latest four or five weeks after birth, sinks under a violent attack of cyanosis, or bronchitis, or from the effects of the fever and atrophy.

The best method of preventing the organic disease of the lungs is, 1st, on no account to hasten the birth unnecessarily, because this never can be done without endangering the child; and 2d, as far as possible to prevent too violent pressure on the head. The respiration of the new-born infant should be accurately observed, and the strength of its voice and manner of sucking attended to; and the moment we find the breathing too short, or the voice hoarse or feeble, and plaintive, or that the infant cannot suck properly, we must immediately exert ourselves to set matters to rights. In the first place, the me-

dical attendant should immediately endeavour to blow air into the lungs, till the thorax is properly expanded. The navel-string must not be cut too soon ; the chest and back should be stimulated by friction with the hand or a brush ; they might also be sprinkled with cold water and sulphuric ether, and the soles of the feet well rubbed with a flesh brush. The child should be put in a warm bath, and the friction continued till it is able to cry loud and breathe properly : should these means not succeed, recourse must be had to internal remedies. An emetic consisting of a few grains of ipecacuanha, a drachm of oxymel of squills, or a quarter or third of a grain of emetic tartar, should be immediately exhibited. Afterwards a quarter, or, in urgent cases, half a grain of calomel should be given, for the purpose of producing general excitement by stimulating the intestinal canal, as well as of diminishing the too copious secretion of mucus in the bronchi, and diminishing the cerebral congestion. The repeated employment of warm baths, either simple, aromatic, (by the addition of thyme or marjoram, and calamus aromaticus,) or restorative, (composed of milk or some mucilaginous fluid,) contributes greatly to revive the system, and restore and promote the circulation. Sinapisms of about an inch in diameter are occasionally to be applied to the chest or neck. The most unwearied attention is requisite to restore and maintain the health of the little patient. By the proper application of the above remedies, by avoiding all the *lædientia*, and even by delaying as much as possible every birth that appears to be proceeding too rapidly, a great many lives may be saved : still the disease is a dangerous one from the very first, and requires great care and watchfulness throughout.*

* We beg to acknowledge our obligations to Dr. West, for his translation from the German of this original memoir.—Ed.

ART. VIII.—*A Case in which a large Molar Tooth entered and passed through the Larynx during the Operation of Extraction.* By John Houston, M.D. M.R.I.A., Demonstrator of Anatomy in the Royal College of Surgeons in Ireland. Surgeon to Baggot-street Surgical Hospital, &c. &c.

(Read before the Surgical Society of Ireland.)

JOHN CLARE, ætat. twenty-nine, a servant of the Rev. Mr. Brownrigg, of temperate habits and good constitution, had occasion, in the month of May, 1830, to get the second molar tooth of the right upper jaw extracted. On the first application of the instrument to the tooth, a fragment of the crown was chipped off, and removed from the mouth by the operator. By a second attempt, the tooth was started from its socket; but on being loosened from the claw of the instrument, it suddenly passed down the throat, and was not seen afterwards by either the patient or the operator. Owing to the large size and firm hold of the tooth in the jaw bone, considerable dexterity and force were found necessary for its extraction.

I saw the man about four hours after the operation, when he presented me with the fragment which had been broken off in the first attempt, and gave me the following account of the sensations experienced at the time of the accident, and after its occurrence. He felt at the instant on which the extraction was completed, a momentary, sharp, prickling pain at the top of the windpipe. This was instantly followed by a severe fit of coughing, which soon went off, but recurred again several times without any evident cause, and at each time with less and less severity, until, after a few hours it ceased to produce any further annoyance. The man stated most positively, that he had not, for a considerable time previously to the accident, been subject to any cough, or other affection of the chest; and that at the instant of its occurrence, both he and the dentist attributed the sudden accession of the symptoms, to the passage of the bone down his

throat. He complained, also, that from the time of the performance of the operation, he could not get rid of a feeling of undefinable uneasiness in the chest ; a sensation of weight in breathing ; and a tendency to draw heavy sighs, which haunted and which kept his mind in a continual state of inquietude. Occasionally, but not at any regular intervals, he coughed up a little frothy mucus, perfectly untinged with blood or purulent matter. He had no hoarseness, no acceleration of breathing though he had walked nearly two miles, no pain in any part of his chest, and no relative difference in the facility with which he inspired, or breathed out the air.

In a consultation with several eminent professional gentlemen, held on him twenty-four hours after the accident, his condition was found to be the same as that just described, and the following stethoscopic signs were noted down : there was a mucous rattle in the lower part of the trachea, audible even to the naked ear, but very distinct when heard through the stethoscope. Both sides of the chest gave a perfectly and equally clear sound on percussion ; but notwithstanding their similarity in this respect, there was a marked difference in the intensity of the respiratory murmur—the sound of the air entering into, and expanding the right lung, being obviously more feeble than that heard at the same moment in the left. There was, likewise, under the right clavicle a slight sonorous rale, a deviation from the natural sound of breathing not discoverable in any part of the left lung. These signs were fixed and not modified or removed by any alteration in the position of the body ; nor by causing the patient to expire with violence, or to take a full breath. In the consultation, various opinions were delivered respecting the course of the tooth, viz. whether it had entered the trachea, or passed into the œsophagus. Some gentlemen, whose faith in stethoscopic signs were not, at that time, (now nearly four years ago,) so strong as it has become since, placing little weight on that part of the investigation, and relying solely on other symptoms, could not bring themselves to

believe, that a large, splintered, and doubly fanged tooth, could find so easy an entrance through the rima glottidis, and lie in the trachea, or bronchial tube, without causing distress of a more urgent nature, than that complained of by the patient; seeing that so small a body as even a cherry stone, can produce so much disturbance there. The fit of coughing, induced at the moment of the accident, might, in their opinion, have been caused by a scratch of the tooth on the epiglottis, while passing through the pharynx into the œsophagus; and it was considered that the transient existence of this symptom, gave confirmation to such a view of the matter. Then, as to the feeling of uneasiness, and tendency to draw heavy sighs which the man complained of, they were accounted for by supposing them to have arisen more from the alarm produced by the marked attention paid to the patient, on account of the singularity of the accident, than from any serious injury he had sustained.

On the other hand, it was urged by those present who happened to be greater believers in the value of the stethoscope, that the fact of the respiratory murmur being feeble and indistinct in the right lung, whilst the sound on percussion thereon was equal to that produced by striking over the left, taken in connexion with the rattle in the lower part of the trachea and top of the right lung, indicated the existence of an obstructing body in the right bronchus. They were ready, however, to admit, that of these symptoms, no single one, nor indeed all taken together, could, of themselves be considered unequivocal evidence of the presence of the tooth in that situation; but, when viewed in conjunction with the other immediate consequences resulting from the operation, in an individual, too, previously in good health, viz. the sudden and certain disappearance of the tooth; the violent fit of coughing which occurred at the instant of its extraction; and the uncomfortable and oppressed state of breathing remaining after, all in their opinion seemed to demonstrate the presence of the suspected body, either in whole or in part in some of the air-passages, and

most probably in the right bronchial tube. The case, however, being of a very unique description, and the evidence in proof of the presence of the foreign body in the windpipe not being, in the minds of all, sufficiently decisive, it was determined, that for the present, no hazardous operation could, with propriety, be undertaken. The man was accordingly placed in hospital, in order that farther observations on his condition might be made.

Not having had from this period more than occasional interviews with the patient, as he had passed from under my care, and not being in full possession of the changes and treatment he underwent, I must content myself with stating, that he passed successively through the several stages of pneumonia, bronchitis, and pleuritis, first of the right, and secondarily of the left lung; that the early period of the illness having been allowed to pass over, without the performance of any operation for its relief, in consequence of the uncertainty and obscurity hanging over the case, it was deemed prudent not to make any unavailing attempt when the prospects of benefit from an operation had passed away; and finally, that he died on the eleventh day from the occurrence of the accident.

The great interest excited by this case on account of the novelty attached to it, and particularly on account of the difference of opinion respecting its nature, gave rise, of course, to a desire among all persons acquainted with its occurrence, to know the result of the post-mortem examination. Numbers were assembled to witness the opening of the body, when, unexpectedly, the friends refused their assent. No persuasion, no entreaty, no bribery, could change their determination; and accordingly the corpse was removed from the hospital. However, having watched the procession, and traced the body to the house in which it was laid, I at length, through the persuasion of the Rev. Mr. Brownrigg, and by his consenting to remain present, obtained leave to make the wished for examination.

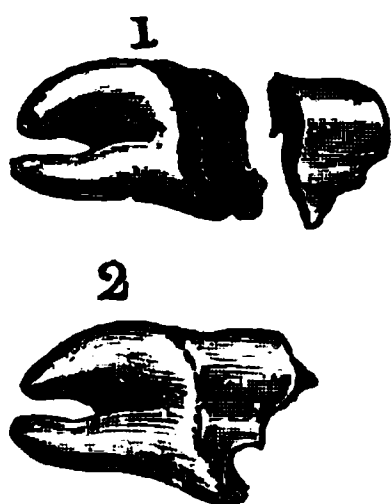
The following is an account of the post-mortem, as noted down at the moment of its performance.

Examination sixteen hours after death.—When the anterior part of the chest was removed, and the pleura opened, the lungs retained their natural fulness; no collapse on either side took place. The right lung was adherent on every side to the parietes of the chest, except posteriorly, where a considerable quantity of thin bloody fluid lay between the pleuræ. The pleural cavity on the left side was also obliterated by an universal adhesion. The lymph effused between the pulmonary and costal pleuræ of the right lung was in considerable quantity, firm and opaque, and evidently of several days standing; that especially, where the fluid noticed above was lodged, for an extent of seven or eight inches, was very abundant and tenacious. The lymph which caused the adhesions on the left side was in smaller quantity, and appeared only as a thin semi-gelatinized stratum. In both, the pleura pulmonalis, when stripped of the artificial membrane, exhibited a highly vascular surface; on the base of the right lung it was red even to lividity. All the adhesions were, however, easily broken through, by pressure with the hand; they were obviously the product of the recent attack of inflammation.

The substance of the right lung was hepatized in every part; its structure readily gave way under pressure with the finger: and when cut into, the surfaces of the section discharged a quantity of serum and dark fluid blood. The left lung, though less advanced in disease, exhibited all the marks of intense inflammation, its integral structure was dense, heavy, and swollen with engorgement.

Such was the condition of the pleura and pulmonary tissue; the next and most interesting part of the dissection lay in the examination of the air-passages. On slitting open the trachea from the larynx down to the lung, the obnoxious tooth was discovered lying in the right bronchial tube, about one inch beyond its commencement; the fangs were directed to-

wards the lung, the broken crown looked up towards the larynx. The bone lay loose in the tube, and came away readily when caught between the points of the scissors. It had two long fangs, and when tested with the splinter, which had been broken off in the first attempt at its extraction, and presented to me by the man while alive, was found to fit most accurately to it, and make a perfect tooth.



1. The portion of tooth found in the lung, with the fragment at a short distance.
2. The tooth and fragment, of the natural size, joined together.

The mucous membrane of the air-passages, from the larynx to the finest ramifications all through both lungs, was in a state of intense inflammation ; it was tumid and softened in consistence, and of a deep red colour, as if injected, The tubes were occupied by a muco-purulent fluid ; but there was no abscess around the tooth, nor even did the structures in the immediate vicinity of the foreign body exhibit a much higher degree of inflammation than those which were more remote.

None of the organs in the chest presented any marks of the existence of previous disease. The pleuritic adhesions were not of longer standing than the period occupied in the course of the late fatal attack ; and there were neither tubercles, nor tubercular cavities in any part of either lung.

This case deserves to be recorded for several reasons :—*First*, on account of its rarity. *Secondly*, as it shows that a body, apparently much larger than the aperture of the rima glottidis, and one even of different form, can find a passage

through that fissure. *Thirdly*, because it proves that so obnoxious a foreign body as a full grown molar tooth may lie for a time in the windpipe, without being productive of much inconvenience. *Fourthly*, on account of the additional evidence which it supplies of the justness of Mr. Key's statement, that the right bronchus is the usual resting place for foreign bodies, which have passed the larynx. *Fifthly*, on account of the practical observations made soon after the accident on the state of the respiration, by percussion and the use of the stethoscope. And *sixthly*, because by the *sectio cadaveris*, the character of inflammatory action induced by the presence of a foreign body in the bronchial tubes is demonstrated.

Respecting the first of these positions, viz. the singularity of the case, nothing such to my knowledge is on record. It is probable that such an accident was never dreaded by any one setting about the extraction of a tooth: and as the like may never occur again, the registry of it may rather be placed among the "cas rares," than held out as a caution against the practice of tooth drawing.

Secondly: to understand aright how a body of greater apparent dimensions than the rima glottidis could have found a passage through that aperture, it is only necessary to reflect for a moment on the nature of the process of inspiration. The introduction of air to the lungs with every breath, is consequent upon the enlargement of the chest—the weight of the atmosphere pressing the adjacent column into the cavity with a rapidity proportioned to the suddenness and extent of the dilatation, and with a force sufficient to carry along in the current any light moveable body, which may happen to come in the way. A small body so intercepted, will readily pass with the air through the rima, and be lodged in a part of the tube lower down. A body of inordinate dimensions may stick so firmly in the aperture, that the full weight of the atmosphere is unequal to its propulsion onwards, and death from suffocation will be the inevitable consequence, if the foreign body be not

instantly shot back again by a powerful expiratory effort, or removed by operation. And, a body of intermediate size, viz. one of such moderate dimensions, as to be capable of passing through the rima, by stretching and divaricating the sides of that aperture, may, when pressed heavily by the atmosphere, as it would during a forced inspiration, be driven past the obstruction, and thence into the trachea or bronchial tubes.

Such latter was, no doubt, the mode by which the tooth in the case above related, found a passage into the bronchus. The man, holding his breath during all the time of the operation, suddenly at the moment in which the extraction was completed, took a full inspiration; upon which, the tooth, partly by its gravity, (the head being at the time thrown back,) and partly carried by the air rushing to the windpipe, fell over the aperture leading into that tube. The obstruction caused thereby to the further entrance of air, induced a spasmodic increase of action in the muscles of inspiration, and a consequent increase of pressure by the air at the opening, by which the tooth was driven with force through the fissure.

Thirdly: as to the freedom from pain or irritation from the presence of the foreign body, during the early part of the patient's illness, it constitutes an important fact to be borne in mind in making a diagnosis in such cases: the absence of this sign had certainly much influence on the diagnosis of the case in question. It has been long ago observed by Louis, and the accuracy of the observation has been since confirmed by others, that foreign bodies in the air passages produce more irritation when lying in the neighbourhood of the larynx, than when low down in the trachea; and that according as they are carried up or down by the air in breathing, so will the patients be afflicted with cough and dyspnoea, or have freedom from such sufferings. The accuracy of the observation receives additional proof from the history of this case. The tooth, a large heavy body, not admitting of elevation by the expired air, lay uninterruptedly in

the bottom of the trachea, and, as Louis significantly observes, "lay easy."

Fourthly: the case here related tends to confirm the opinion now generally entertained, that the right bronchus, in preference to the left, is that usually occupied by a foreign body carried by the air through the trachea. It was into this part that Mr. Key found a sixpence to drop, when let fall in the dead body through the rima glottidis; and it was here that he discovered the sixpence which caused the death of the individual whose case led to the performance of such experiments. The multiplication of proofs in favour of an important pathological fact, and one which may lead to greater accuracy of diagnosis, and greater precision in the performance of any operation, undertaken for the safety of human life, is of great value; and it is obvious, that such must be the tendency not only of Mr. Key's highly interesting communications, but of that just offered by me in corroboration of his statements. With respect to the possibility of saving, by an operation, the life of the individual in question, no more can at present be said, than that some attempt would most likely have been made, had the presence of the tooth in the right bronchus been clearly ascertained; and that the forceps recommended by Mr. Key, consisting of two long narrow blades, capable of being passed down the trachea through an artificial opening, might have been those to which a trial would have been given. It is clear, that the ordinary operation of making an opening in the wind-pipe, through which the offending body might be expelled by the efforts of expiration would not have been sufficient. The tooth should have been actually lifted from the right bronchial tube by mechanical means; otherwise its removal could not have been accomplished. The weight of the body would have opposed its elevation from the bronchus by any force which the expired air could exert upon it; and its size and irregularity of form would have been unfavourable to its discharge through such an opening as the calibre and connexions of the trachea would admit of.

Fifthly: the application of the stethoscope to the detection of foreign bodies in the air passages, is a recent improvement in the use of that valuable instrument. The absence of respiratory murmur in one lung during inspiration, when, by percussion, it is proved that the cells of the organ are healthy, and filled with air, is, in a case of this nature, a clear proof of the presence of an obstructing body in the bronchial tube leading to the affected organ; and the circumscribed mucous rale, audible in the same place, may be considered as indicative of an increase of secretion, caused by the presence of the local irritation. These phenomena, so intelligible consequences of the accident, and so characteristic of its nature, were first observed in this city, in two patients in the Meath Hospital, and their presence alone justified and led to the performance of successful operations for tracheotomy.*

Sixthly: the pathological condition of the thoracic organs of this patient presented some points of interest. The morbid changes induced by the late inflammation, were not all of equal standing; they had, on the contrary, commenced in different parts in succession, and might be traced as from a centre, referrible to the right bronchus the seat of the tooth—through the mucous membrane, pulmonary tissue and pleura, first of the right, and subsequently of the left lung, until all these textures were involved in the same disease. Ordinary instances of bronchitis induced by cold, are not prone to run rapidly into pneumonia, and much less to spread to the serous membrane covering the lung; the case, however, appears to be very different where the inflammation of the mucous membrane is caused by the presence of a foreign body, as is shewn by the dissection of the lungs in the individual, the subject of this communication.

* These two highly interesting cases are recorded in the 5th vol. of the Dublin Hospital Reports, by Professor M'Namara.

ART. IX.—*On the occasional Occurrence of Mental Incoherence during Natural Labour.* By W. F. MONTGOMERY, M. D., Professor of Midwifery to the King and Queen's College of Physicians in Ireland.

PERHAPS I cannot more appropriately introduce the remarks which I have to offer on this subject, than by quoting, as a preliminary observation, one prefixed to a highly instructive and valuable paper on vaccination, lately published by Dr. Maunsell in this Journal, his words are : “ a very limited experience must be sufficient to expose to any thinking person the fallacy included in the supposition, that the difficulties and perplexities of a practitioner are chiefly to be met with in the more dangerous and rare cases of disease.” Now, if there be one subject more than another, to which such a fallacy is found to extend its influence *injuriously*, it is that of natural labour, the laws, circumstances, and phenomena of which are usually considered as matters of such constant and common-place uniformity, that it is deemed quite enough to have ascertained, that a labour is natural, to enable us to anticipate every symptom that will arise, and understand every circumstance likely to occur during the whole of the process ; in short, that as soon as we have *been present* at a few cases, we may with a safe conscience rest satisfied, that we know all that is to be known of the matter, and adopt the comfortable maxim, “ *ex uno disce omnes*,” a rule of discrimination which we might, with just as much propriety, attempt to apply to the human head and face, and assert their universal similarity, because there is in all an assemblage of the same parts ; but even in those which are to the careless observer indistinguishably alike, how easily does the quick and practised eye of the anatomist detect imperfection of structure or irregularity of arrangement, or the phrenologist or physiognomist recognize the characteristic differences which distinguish the philosopher from the sensual voluptuary or the vil-

lain ; so also will the skilful painter discover and appreciate varieties in proportion or expression, which as perfectly distinguish one individual from another, as if they belonged to different species ; and by such nice discrimination of these more delicate points, and his power of treating them correctly, does he earn for himself well-merited praise and high reputation ; and so it is or ought to be in our profession ; and my belief is, that there are not to be met with any two cases of labour, which, if carefully observed, will be found precisely alike ;* nor any one case which, when attentively watched, will not present to us some interesting variety of circumstance, or suggest some new idea, thereby adding to that stock of knowledge, which is to render us afterwards more certainly useful to our patients, and consequently assist us in advancing to professional eminence, by means which are at once the most creditable, safe, and certain. It is by no means my intention to trespass on the time of the reader or weary him, by even an enumeration of the symptoms or aggregation of circumstances, which usually present themselves in natural labour, with which, I will take for granted, that he is already perfectly familiar ; but I may be permitted to advert to one fact vitally connected with our subject, for the purpose of shewing how slowly some, even of the circumstances apparently easily known, became objects even of consideration, much less of knowledge. I allude to the relations which the head observes with regard to the pelvis, as it descends through that cavity and issues from its outlet : now, strange as it may appear, it is nevertheless not more strange than true, that although men of great talent and observation were employed in the practice of midwifery, and distinct treatises were written on the subject, so far back as the beginning

* Such also, and even more decided, was the conclusion to which Denman's experience and talents led him ; " we may," says he, " probably never meet with any two labours in any respect exactly similar." p. 284. ed. 5.

of the seventeenth century,* when the works of Ambrose Paré were collected and translated, when the great Harvey practised midwifery, and wrote his "*Exercitationes de partu*," and when Ruysch was elected Professor of Midwifery by the States of Holland, still no idea was entertained of the true mechanism of the passage of the head, nor was even an attempt made to explain it, until the middle of the eighteenth century, when Sir Fielding Ould, then master of the Dublin Lying-in-Hospital, published his view of it in 1742;† in fact, we had no full, clear, and satisfactory account of it, until Nægelè of Heidelburgh published his "*Mechanism of Parturition*" in 1829.‡ And yet I need not here insist, that on the correct and intimate acquaintance which we possess of these relations, and our expertness in detecting them by examination, depends altogether our capability of managing or correcting malpositions, and of applying and using instruments with advantage and safety. Still I regret to believe, that there are many, very many who consider it quite sufficient to have ascertained the presenting part to be the head, to satisfy them that all is right, and who, to use the words of Dewees, "commit the whole charge of the labour to the management of nature, if the head present, *no matter how*, and as she is usually successful, however hard the struggle, never stop to inquire whether they could have aided her efforts or abridged her toils;"§ never considering

* I am aware, that there were publications of even earlier date, as for instance, "*The Byrthe of Mankyade, or the Woman's Booke*," in 1540, but they are not of any authority.

† Before that period it appears to have been taken for granted, that because the head issued from the pelvis with the occiput towards the pubis, and the face to the sacrum, it passed into and through the pelvic cavity in the same position, which was accordingly designated as the "*situs capitis rectus et equus*." See figure in Mauriceau, vol. i. edit. 1712, p. 243, and also Deventer's 8th and 9th plates, French edit. 1734, pp. 95-6.

‡ Nægelè's book was printed in Germany in 1822, but it was not known in this country until the publication of Dr. Rigby's translation in 1829.

§ Compendium of Midwifery, page 237, par. 577.

that there are at least five varieties of relation between the head and the pelvis, each of which is accompanied with a corresponding difference in the circumstances of delivery, and, of course, may require a very different mode of management.

If in dwelling even thus long on matters so familiar to most of my readers, I have trespassed on their patience, I can only urge in extenuation, the extreme importance of the subject of natural labour, which Denman tells us, “was the last thing well understood in the practice of midwifery;”^{*} and the engrossing interest with which it is invested, as being not alone a delicate branch of professional inquiry, but also a process whose results so powerfully influence our domestic happiness, and are associated with some of the purest and tenderest feelings, which spring from and cement the most sacred of our social relations.

I shall now proceed to notice the particular symptom of incoherence during natural labour, to which I wish to call attention; and I am the more anxious to do so, because, as far as I am aware, it has not been described by any writer on midwifery. It is well known, I presume, to every one who is conversant with the management of labour, that the period of that process at which the os uteri becomes fully dilated, and is put severely on the stretch by the head passing through it, is one of extreme distress and pain to the patient, who generally at that time is much agitated, experiences a smart rigor, and vomits; but in addition to these and other symptoms of almost constant occurrence, I have observed, that the impression thus made on the system is in so many instances accompanied by incoherence or temporary delirium, that I have ceased to regard its occurrence as a matter of surprise; and yet I confess, that I was six years engaged in the practice of midwifery, having also attended the Lying-in-Hospital for two years and a half, before I took notice of this fact, which was at length

^{*} Introduction to Midwifery, ed. 5, p. 268.

presented to my observation in characters so forcible, as not to be overlooked or easily forgotten: since then, I have watched closely, and have frequently met with it; sometimes strongly marked, at others so slight as almost to escape observation; but perhaps the best description I can give of this circumstance, will be collected from a brief detail of some of the cases in which it was observed. I shall therefore select three out of those in which it occurred under my own observation, and three others which were communicated to me in consequence of my having discussed the subject in my lectures.

CASE I.—A. S. in labour of her second child, of very irritable temper, was going on favourably, and without any symptom calculated to excite attention, until the rupture of the membranes, which took place before the os uteri was fully dilated, the pains increased in strength, and the head began to press hard into and stretch the os uteri: in a few minutes afterwards, on my asking her some indifferent question, she popped her bare leg out of the bed, and requested that I would unlace her boot and take it off, for that it made her very uncomfortable, and was insupportably hot in such warm summer weather; then a pain or two passed over, and she again reiterated the request about my taking off her boots, or that I would allow the nurse to do so, and if not, that I would at least pour the contents of the water jug over her to cool her; then two or three pains more passed over, and she started on a new theme, “ Doctor, don’t you think Mr. — is a very handsome man, I think him a charming man, I wish I was married to him, and S—— might marry any one he liked.” The gentleman whose name she mentioned, was, perhaps, the ugliest man to be found any where: all this lasted about a quarter of an hour; the head then passed the os uteri, and she presently dozed a little; the labour afterwards went on for several hours, and rather severely, but she had not the slightest return of the incoherence, and recovered well. This was the first time I had observed this occurrence, and it alarmed me exceedingly.

CASE II.—Mrs. D. aged 25, of very irritable habit, and rather ardent temper, sent for me at ten o'clock, P. M. ; it was her first labour, and was proceeding actively with powerful pains. At half past one the membranes ruptured, and the head began to bear upon and into the os uteri ; at two o'clock, when this part was fully on the stretch, and she was covered with perspiration, she turned to me and said, that she “ would not go down to the drawing-room ; for what would visitors think if they saw her sitting there, and she in labour ? and that I need say no more on the subject, for that she would not do it, but that she would have no objection to step into the coach and start off for Ballybay ;” here she concluded, began to doze a little, and wandered no more ; the head soon after came down upon the perineum, and a little after three o'clock she was safely delivered of a very large and healthy boy, and recovered well. I since attended this lady, and she had no return of this symptom.

CASE III.—Mrs. S. aged about 40, remarkably healthy and even tempered ; in her ninth labour, which began on the 14th by the dribbling away of the waters, she had a few slight pains in the course of that day, but the labour did not increase or become active till nine o'clock, P. M. of the 15th, when the pains were quick and forcing, and about twelve o'clock midnight, she turned to me and said, that she “ thought it would be time for her to get up and see the children dressed and sent to school, otherwise they would be late ;” then she expressed great surprise as to what could be preventing her mother from coming to town, to be with her during her confinement, which, she said, she knew would take place before her arrival ; her mother was at the moment standing by her bedside and holding her hand : presently the head descended fully into the cavity of the pelvis, and the incoherence ceased after having lasted about five minutes. She was safely delivered at twenty minutes before two o'clock, A. M. of the 16th.

CASE IV.—An extremely intelligent gentleman, son of a physician in England, who was attending my lectures in

1831, told me, that my account of this matter was very satisfactory to him, as explaining a circumstance which had happened in his father's practice, which had been at the time of its occurrence a source of extreme alarm, and had always appeared to them both totally unaccountable. He had been attending a lady of rank in labour, she was remarkable for the gentleness of her disposition and propriety of conduct; every thing appeared to him to be going on perfectly well, and the labour promised to be speedily terminated, when the lady, in reply to some question from the doctor, desired him to go be damned for a scoundrel; and then followed up this singular address by language not less remarkable for its indecency, than for the inappropriate circumstances under which it was introduced; this, however, lasted only a few minutes, when she became quite calm, and observed, that she believed she had been talking nonsense, but did not know of what kind: her labour was happily terminated in a very few hours, and she recovered well; but from that time, Dr. — and his son always considered her as a person liable to insanity, until the latter heard me describe such occurrences as not unusual.

CASE V.—The account of this case was very kindly communicated to me by Dr. Young, of Ormond-quay, in consequence of hearing me lecture on the subject. I shall give it in his own words:—

“ Upper Ormond-quay,

“ MY DEAR SIR,

“ Agreeably to your request, I furnish you with an outline of a case which fell under my observation, and which you seemed to think deserving of attention, as far as regards a peculiar symptom, which has not, I believe, been noticed by practical writers. The symptom I allude to is, the *slight* but *marked delirium*, which sometimes attends natural labours, just as the head of the foetus is passing through the os uteri, which is then forcibly distended and exceedingly on the stretch.

“ On the 14th of March I was called upon to see Mrs. R. H. of Mabbott-street, aged 20, then about to be confined with her first child. The labour was perfectly natural, and continued active for at least eight hours; I thought it necessary to leave her room for a few minutes, and on my return, I found that the membranes had been ruptured, which I attributed to the interference of the nurse. The pains now became more violent, the head forcibly pressing through the os uteri, when at this moment I was greatly alarmed by her *incoherent expressions*, (as if in conversation with her relations, who were at that time in the country,) and was induced to suppose, that these were the premonitory symptoms of an attack of convulsions; but I was agreeably relieved in about two minutes from my great anxiety, by the complete subsidence of these (as I then thought) alarming symptoms; in fact, the child was entirely expelled after a few pains, and the lady's recovery was uninterrupted.

“ Faithfully your's,
“ EPH^s. YOUNG.”

CASE VI.—For this case I am indebted to Dr. Paterson, of Blessington-street, whose account of it I shall transcribe, together with his very judicious preliminary observations:—

“ 32, Blessington-street.

“ DEAR SIR,

“ As the reputation of the physician is as much endangered by being alarmed without cause, as from not observing danger when it really exists; and as in the practice of medicine, a knowledge of the *causes* of disease in general, renders our prognosis clear, our treatment decided and efficient; so in that of midwifery, *every fact* founded on observation, which elucidates the *causes* of symptoms, and which tends to prevent unnecessary alarm in the mind and conduct of the medical attendant, ought faithfully to be recorded. In accordance with this sentiment I send you the annexed case, in whose first

stage symptoms occurred, which, during their continuance, occasioned me considerable anxiety.

“ In the month of October, 1831, Mrs. S., a lady aged 30 years, of a strong but nervous constitution, pregnant of her first child, and at the full period of utero-gestation, was at two o'clock, A. M., awoke with the precursory symptoms of labour. There was neither headach nor fever, and little, if any excitement of the pulse, but she was now, as she had been during the greater part of gestation, unusually anxious and desponding as to the result. Her diet, exercise, and bowels, had been carefully regulated, and every proper means to remove her apprehension adopted. The os uteri was contracted and *slightly rigid*; an emollient enema was administered, and rest—sleep if possible, advised.

“ Five o'clock, A. M. She had not slept, but expressed herself soothed by the enema which produced the desired effect; the pains were sharp and frequent; the voice characteristic, and the os uteri now dilated to nearly the size of a shilling; she complains much of *cutting* pain in her back; no headach; pulse quiet; little thirst. Her bed-chamber was large, well ventilated, and cool; no fire nor any stimulant allowed.

“ Seven o'clock, A. M. Os uteri dilating gradually though slowly; when my patient began to talk incoherently, and ultimately to sing a favourite Italian air, which, I understood, she had been playing the day previous. She had spoken collectedly the minute preceding; from this *error mentis* she was at once recalled, by applying cold to her forehead and speaking mildly to her, when she expressed herself aware of having “ done or said something foolish,” in the interval she complained of the lancinating pains, and again resumed the wild melody; from which I always with ease recalled her. Alternate moments of sensibility and delirium occurred, till half past eleven o'clock, A. M., when the os uteri being almost fully dilated, the membranes and head presenting, the tendency to wander ceased, and though the external parts did not dilate

kindly, the delirium did not again recur ; her recovery was favourable.

“ I candidly acknowledge I was at first alarmed ; but the quiet state of the circulation ; the freedom from fever or local determination ; the ease with which I recalled her to perfect recollection and sensibility ; by leading me to consider the incoherence as the effect of pain in a highly hysterical individual, (as my patient undoubtedly was,) allayed my fears, and enabled me to think with coolness and to act with deliberation. I have not mentioned what perhaps I should, that I could not *then* account why the delirium, if arising from the pains, did not continue as long as they did, nor did I distinguish between the effects of the pain of dilatation of the os uteri and of expulsory action, till my conversation with you on the subject gave me reason to attribute the mental affection to the former.*

“ Very truly your’s,

“ HENRY PATERSON.”

Such are the characters and accompanying circumstances of this species of incoherence, which it appears to me of great importance to have known, and properly appreciated, especially by those who have had but little experience in practice, but who may have learned, that delirium in labour is to be regarded as a dreadfully dangerous occurrence, which it undoubtedly is, when it comes in the train of other bad symptoms, accompanying a difficult and protracted case ; but this form of incoherence of which I speak, has no affinity with such a condition ; it comes on suddenly, during perfectly natural and favourable labour, and most frequently at the particular stage of the process which I have pointed out ; it is not accompanied or followed by any other unpleasant or suspicious symptom ; it occurs, perhaps, immediately after the

* This lady was since confined a second time under Dr. Paterson’s care, without any return of incoherence.

patient has been talking cheerfully, and having lasted a few minutes, disappears, leaving her perfectly clear and collected, and returns no more, even though the subsequent part of the labour should be slower and more painful. In every instance which came under my observation, the patients were afterwards conscious that they had been wandering, and occasionally apologized for any thing wrong they might have said, although they were not aware of what the exact nature of their observations might have been, their condition, so far, exhibiting that mental embarrassment observed by Dr. Gooch,* as occasionally occurring "in women who had been debilitated by nursing," in which "the mind was wrong, yet right enough to recognize that it was wrong." Now should the practitioner in attendance not happen to be aware of this fact, his fears would almost certainly be alarmingly excited, and might, perhaps, induce him to pronounce an opinion, or adopt a course of treatment, under the apprehension of imminent danger, which the subsequent circumstances of the case would not justify, and which might even be highly injurious. Thus, one gentleman states, that he was sure his patient was about to be seized with convulsions, and had the symptoms continued, he would probably have felt himself called on to bleed her, or request further advice, neither of which is desirable when unnecessary: another told me that he fully anticipated the approach of puerperal mania, and a third, that although from the time of the occurrence, he never recognized any further symptom of derangement, he had always considered the lady as of unsound mind, until I mentioned to him what I had observed; and precisely the same distressing impression was made on the husband and family of another lady, until I relieved them by describing the circumstance as one which I had several times met with, and to which I attached no consequence; an assurance which was received with almost a transport of delight and satisfaction. I have al-

* Account of the Diseases of Women, p. 114.

ready said that this occurrence is not described by authors, and I did not venture to make that assertion until I had carefully examined the works of Denman, Burns, Merriman, Gooch, Hamilton, Smellie, Dewees, Gardien, Velpeau, Desormeaux, Baudelocque, Boivin, Mauriceau, La Motte, Deventer, and Stein, of whom, only two* mention such an occurrence at all, and they merely state generally, with regard to labour, that sometimes, when it is violent and under peculiar circumstances, (which they do not describe or specify), the woman "becomes delirious, or suffers a transient disturbance of the intellectual functions;" but they have given no account of the form such incoherence assumes, its distinguishing characters, or the accompanying circumstances; nor have they or any body else, as far as I know, reported any case calculated to call attention to the subject; and although I find, upon inquiry, that it has occurred to other professional men, (as in the cases already related,) they stated to me at once, that they recollected it only as having greatly surprised and alarmed them, and that until they heard my account of it they had never thought of connecting it with any particular cause, or part of the process, or in fact had regarded it as anything more than one of those strange and unaccountable incidents which occasionally spring up to astonish and embarrass us in practice.

I should also observe, that I have known the same circumstance happen in abortion, when the ovum was forcing through the rigid cervix and os uteri; but more frequently the effect then produced is extreme faintness, and a degree of exhaustion totally disproportioned to any appreciable cause, such as hæmorrhage. I lately attended a lady, who was miscarrying in the second month, with slight hæmorrhage, and during the distention of the lower part of the cervix uteri, such a degree of faintness and exhaustion prevailed, that she was almost

* Gardien, *Traité d'Accouchmens*, tom. ii. p. 224, and Desormeaux, *Dict. de Med.* tom. i. p. 170.

completely insensible for more than an hour; but no sooner was the ovum expelled into the vagina and removed, than she revived again and felt quite well, a very satisfactory proof that the symptoms of exhaustion had been the result of the peculiar impression made at the time on the brain and nervous system. The subjoined fact also appears to me of considerable interest, as illustrative of our subject. A professional friend informed me, that an attempt made by him to dilate the os uteri for the cure of dysmenorrhoea, as recommended by Dr. Mackintosh, was accompanied by excessive distress, and such complete incoherence on the part of the patient, that he was afraid ever to renew the operation.

It should not be forgotten, that there are many other circumstances connected with pregnancy and parturition, which are so much in unison with the occurrence of the symptom now spoken of, as to render its appearance a matter of less surprise or singularity. It is well known that in almost all women, but especially in those of an irritable habit, conception is followed by very obvious alterations in their moral temperament, apparently depending on a state of exalted nervous sensibility, or, as Denman* well expresses it, "in consequence also of this general and perpetual irritation, the temper of pregnant women is sometimes rendered less gentle and patient, than is consistent with their usual character, and this claims compassion instead of resentment;" in some the irritability assumes the extraordinary and protean forms of hysteria, and occasionally the patient's condition is such, that it would be no easy matter to draw the line of separation between it and absolute mental aberration, or, at least, what is usually called an unsettled state of mind; while in others complete derangement takes place. A patient of my father-in-law's, the late Dr. Connor, became deranged in eight successive pregnancies, and very lately a woman was received into the Richmond Lu-

* Introduction to Midwifery, p. 216.

natic Asylum, who was three times similarly affected during gestation, but always became sane a short time before delivery, and continued so until the re-occurrence of pregnancy. It is also a fact universally acknowledged, that the nervous system of all *parturient* women, is in a state of peculiar excitement and impressibility, by which they are rendered most painfully sensitive to the slightest moral or physical disturbances, and the very circumstance of the dilatation of the os uteri in labour, is in some a source of irritation sufficient to induce that fearful affection of the brain, which we believe to constitute in many cases the essential cause of puerperal convulsions; and the appearance of mania during the state of child-bed, is of too frequent occurrence to require more than to be mentioned. Nor shall we be at a loss for some very close and striking analogies if we leave this particular system altogether, and turn our attention to other organs and their functions: thus it must be familiar to many, how frequently children become incoherent, especially at night, after having paid a visit to the pastry cook's shop, and received into their stomachs a quantity of indigestible matters; an effect which I once saw remarkably exemplified in an adult: the gentleman was then a medical student, went to the theatre, and after his return supped on lobster and mutton kidneys, after which he drank two or three glasses of wine and nothing more; the following morning he got up as usual, and went to a gentleman who was preparing him along with others for his examination; he was, in general, as I was informed, remarkable for the extent of his information and good answering, but on this occasion his replies were the most unqualified nonsense: in consequence of which, he was advised to return home and rest himself; which he did; but shortly after his arrival at his lodging, I was sent for in great haste, in consequence of his having fainted. On his recovery I asked him if he felt sick? he said not: I asked if his bowels were out of order? he said not; that they were confined, but that if he had diarrhoea it would cause an effusion of coagu-

lable lymph, which would put an end to the insensible perspiration: he was very cold and shivering. I got him into bed, gave him a little warm wine and water, and applied heat to his feet. While these measures were in progress, I ascertained what I have already stated, about the supper he had eaten, which it seemed also was a very unusual thing with him to do, as he was of remarkably temperate and regular habits; his tongue was foul, and his abdomen felt full and doughy, as if the bowels were much loaded. I ordered an active purgative, to be followed by an injection. In a few hours afterwards, I had the advantage of Mr. Colles' advice, to whom he was serving his time; he advised the aperients to be repeated, as the former dose, &c. had acted only slightly: he was quite aware that he had been talking nonsense to us in the morning, for which he apologized; but he was, nevertheless, still quite incoherent; and a moment afterwards, in answer to a question from Mr. Colles, as to what wine he had drank the night before, he said he had taken "only three glasses of intertrochanteric wine, which you know, Sir, is the proper name for port." In the course of the evening the bowels were relieved plentifully, and the previous night's supper discharged almost exactly in the state it had been swallowed; he got several hours good sleep during the night, and in the morning was as perfectly collected as ever, nor had he ever to my knowledge any return of the annoyance. Since then he passed his examination with distinguished credit before the College of Surgeons.

Dr. Gooch relates a still more remarkable case, in which mania after delivery was induced by disorder of the alimentary canal, and subsided immediately upon the expulsion of the offensive matter from the bowels.*

In conclusion I may observe, that an explanation of this phenomenon seems to me to arise naturally from a considera-

* On the Diseases of Women, p. 156.

tion of the anatomical structure of the part principally engaged, the changes which it undergoes at the time, and the physiological condition of the uterus resulting from pregnancy. With regard to the former, we know that the lower part of the cervix, or that surrounding the os uteri, has the structure, and performs the functions of a sphincter, and that forcible or extreme dilatation of such parts is always productive of great distress ; and in the present instance, we have an aperture, which, this hour, would hardly admit the point of our finger, before the end of another hour, dilated to such a degree, as to give passage to a body, measuring at least from ten to twelve inches in circumference ;* in doing which, it is sometimes so stretched, as to tear in several parts, and thus gives rise to the jagged or notched state of the part, so constantly found in women who have borne several children. I think also, I have sufficient evidence to believe that this incoherence will most frequently be observed in cases where the membranes have given way, or have been imprudently ruptured, too early in labour, by which the expulsive action of the uterus suddenly acquires increased force, and impels prematurely, the hard, bulky, and unaccommodating head against the os uteri which it carries down before it, until a portion of the lower part of the cervix is caught and severely pressed between the head, and the bony wall of the pelvis, which greatly adds to the pain and distress experienced, and besides throws the part into a condition still more unfavourable for dilatation. This theory (if it deserve such a name) is fortunately one, which, whether true or false, may be entertained with safety, inasmuch as the conduct to which its reception would lead, could have no other than a good effect, since it merely furnishes an additional precaution

* It may be proper to observe here, that this is not the greatest circumference of the child's head, which is that measured from the occiput, over the parietal protuberances, and round the forehead, which amounts to from thirteen to sixteen inches ; but that is not the one presented to the os uteri in natural labour, in which the head passes in such a position, that its outline is nearly a circle, whose diameter is the bi-parietal or transverse dimension of the cranium, or nearly so.

against a premature interference with the membranes. Lastly, we know that the part of the cervix around the os uteri receives a very abundant supply of nerves from the great hypogastric plexus, and thereby establishes intimate connexions with the branches of the renal and spermatic plexuses; which nervous supply becomes not only physically increased during the time of gestation,* but (in common with the other constituents of the organ) has its functional activity or its peculiar sensibility greatly exalted, manifesting its irritative influence, not only by the derangement caused in the action of certain viscera, as the bladder, stomach, and intestines, but in many instances disturbing seriously the whole system, and inducing effects already adverted to.

Such an explanation appears to me not only reasonable in itself, but the more satisfactory from its corresponding so closely with that offered by Dr. Gooch, to account generally for the disorders of the mind in lying-in women, which is the following: "the sexual system in women is a set of organs, which are in action only during half the natural life of the individual, and even during this half, they are in action only at intervals. During these *intervals of action*, they diffuse an *unusual excitement* throughout the nervous system; witness the hysteric affections of puberty, the nervous susceptibility which occurs during every menstrual period, the nervous affections of breeding, and the nervous susceptibility of lying-in women. I do not mean," he adds, "that these appearances are to be observed in every instance of puberty, menstruation, pregnancy, and child-bed, but that they occur sufficiently often, to show that these states are liable to produce these conditions of the nervous system;"† but he makes no mention, either in his essay on that subject or elsewhere, of the occurrence which we have been here considering, and which appears to me a forcible illustration of the general law just referred to, while, in the

* Vide Tiedeman's *Tabulæ, et Nervorum Uteri Descriptio*.

† *Op. jam cit.*

particular instance before us, we can moreover ascertain the connexion or at least the coincidence between the mental affection and the existence of a powerful impression made, at the time, on a peculiarly sensitive and irritable part of a system, which we know does, even under ordinary circumstances, exert so decided an influence over not alone the physical, but also the moral and mental constitution of the female.

P. S. Should any facts, illustrative of the subject of the foregoing observations, have presented themselves to the reader, I beg to say, that I would consider their communication as a favour.

18, MOLESWORTH-STREET.

ART. X.—*Practical Observations on the Treatment of some of the Diseases of the Lungs, &c.* By ROBERT LITTLE, M.D., Lecturer on the Institutes and Practice of Medicine in the Belfast College. Physician to the Belfast Hospital. Physician Accoucheur to the Belfast Lying-in Charity, &c. &c.

It is more particularly in extensive hospital practice that the physician has it in his power to form a just estimate of the comparative merits of different remedies for the mitigation or cure of disease. The want of an opportunity of observing a sufficient number of cases, has led practitioners of much merit into great errors, respecting the advantages of various remedies, for it is, in fact, only by the most careful comparison of those curative means, recommended as most salutary in the treatment of any disease or class of diseases, that it is possible to assign to each its peculiar advantages; and as the situation of physician to the Belfast hospital, which I have held for several years past, has afforded me every facility of studying practically pulmonary affections, I now think it right to submit to the public a brief account of the result of my experience. The situation of Belfast is remarkably low, and also extremely damp, so that

those pulmonary affections, common to such situations, prevail in it, as might be expected, to a very considerable extent. Pulmonary consumption is certainly of very frequent occurrence here, but that disease is, in all probability, favoured as much by the numerous manufacturing establishments, as by the dampness of the atmosphere. I am led to believe that acute inflammatory diseases do not prevail in a greater degree in Belfast, than in less damp situations during the winter months, but in the spring months, owing to its not being, to any considerable degree, defended from the East and North winds, which usually prevail at that season of the year, I think acute pulmonary diseases occur more frequently, than in almost any town of the same extent with which I am acquainted.

In the treatment of no single class of diseases, has there been greater diversity of opinion with regard to the most suitable curative means, than in that of the pulmonary organs; some practitioners trusting almost exclusively to one plan of treatment, and others, again, relying on that of an opposite nature. This, however, is only what has always been the case, in a certain degree, when the nature of the disease is, as in many instances, incapable of improvement. There are some pulmonary affections that are scarcely susceptible of palliation when they have arrived at a certain stage, but there are others, again, at all times capable of receiving the greatest benefit from suitable remedies. I therefore think it would be quite unreasonable for any physician to recommend the same plan of treatment to be exactly followed, under all the changes that this dangerous class of diseases is liable to present; and although it is my intention, in this paper, to dwell most on the advantages of counter-irritation, still, it will be seen, that I am not insensible to the great benefit of other means.

Turpentine is the counter-irritant I employ most in the treatment of pulmonary affections, more particularly in those of the air tubes, and as it is one that has not been hitherto recommended, as far as I know, to any considerable extent in

these diseases, I will take as extensive a view as possible of its peculiar advantages, when compared with other remedies of the same class.

I was first led to the extensive application of turpentine to the chest in pulmonary affections, from what I knew respecting its advantages, as an antispasmodic and counter-irritant in other diseases: and I found on each trial the most satisfactory results as to its advantages in soothing morbid irritation, and at the same in subduing (in proportion to the extent and duration of its application) internal diseased action. The two counter-irritants in most common use in the treatment of pulmonary diseases, are tartar emetic and cantharides. Now with regard to these, no person can deny, but that they are both capable of producing powerful irritation on the parts to which they are applied. This, however, is not exactly all that is required in the treatment of pulmonary disease, but more particularly in that of the air tubes, when there is much acute inflammatory action present. The irritation of the surface, unless that irritation gives rise to a change in the sensibility of the respiratory muscles, cannot in these affections be productive of more than partial benefit, for in all cases where the respiratory organs are in a state of acute inflammation, there is spasmodic action of the muscles immediately connected with these organs, but more particularly those of the air tubes, and it is very common to find cough, which is merely a spasmodic action of these parts, continue for a considerable time after all inflammatory action has been completely overcome, so that whatever counter-irritating application is used in these cases, should have also, if possible, an antispasmodic effect. I certainly do consider the antispasmodic powers of the turpentine application in pulmonary diseases, of the greatest consequence; but keeping that effect out of view altogether, I would prefer it as a counter-irritant to any other with which I am acquainted.

Sometimes pulmonary diseases are accompanied with great local and general excitement of the system, and at other times

the very reverse of this is the case,—there is great local and general debility. Now I am quite convinced, from much experience in the application of turpentine as a counter-irritant, that it may be carried to any reasonable length, without the least danger of either increasing internal inflammatory action or fever in these cases; or internal or general debility. Tartar emetic is much too slow and too irritating an application, when the pulmonary organs are in a state of acute inflammation, and the application of a blister to the chest is of very doubtful efficacy, even when blood-letting has been premised. I am quite convinced, when these organs are in a state of acute inflammation in children, that blisters are, in nine cases out of ten, more injurious than useful. But such is not the case with the application of turpentine, no matter whether blood-letting be premised or not. The vascular system in children is very easily roused, and this is a thing that I fear is generally kept too little in view, in the treatment of the internal inflammatory affections which I have been considering. When turpentine is applied freely to the chest of a child, labouring under acute pulmonary disease, it soon excites inflammation of the skin, but this excitement of the skin is not immediately succeeded by increased frequency of pulse, and increased difficulty of breathing, as is frequently the case after the application of a blister; but, on the contrary, brings down, in proportion to the external excitement that is produced, the frequency of the pulse, and at the same time affords relief to all the internal inflammatory symptoms. I am therefore well satisfied, that the proper application of turpentine to the chest will be found in many cases of pulmonary inflammation, but especially in that affecting the air tubes, infinitely more useful than either tartar emetic or blisters, on account of its antispasmodic as well as its stimulating effects. I would not merely give a preference to turpentine over all other stimulants when applied to the chest, in acute inflammation of the pulmonary organs, but would prefer it much in those of a chronic nature; because it has no tendency

like the others to increase debility, both local and general, a too common accompaniment of such affections. I am quite convinced, that the purulent discharge that is frequently kept up in these cases by means of tartar emetic and blisters for weeks, nay, sometimes even for months at a time, is productive of the very worst effects in certain cases. When any chronic pulmonary affection has subsisted for a length of time, the system generally is brought to a state of debility, and it is, in nine cases out of ten, making bad worse to bring it down by any curative means still lower. I think I have seen in old age in particular, when the system is altogether in a state of great debility, the worst possible effects from the application of blisters. I have seen, for instance, the bronchial secretion suddenly increase so much by their application, as to give rise to suffocation, and I am certain I have observed the same thing occur in infancy and childhood repeatedly. I have however, applied turpentine to the chest as a counter-irritant at all ages, and in the lowest states of the system, and I can safely say, I never saw any debilitating effects produced by it, no matter to what extent it was carried.

For some time past, the inhalation of chlorine and iodine gases has been much extolled for the cure of chronic affections of the air tubes of the lungs; and the inhalation of the same gases has been even strongly recommended by medical men of great respectability, for the cure of pulmonary consumption. I have given a very extensive trial to these gaseous inhalations, and I am sorry to say, that they have not proved so successful in the cases where I have tried their curative powers, as I had expected from the very strong manner in which they had been recommended by many individuals to public notice. I have certainly found these gaseous inhalations of use in chronic affections of the air tubes of the lungs, but again I have found them, by their stimulating effects on the mucous lining of the same tubes, convert a chronic affection into one of an acute kind. The difficulty in the use of these gases is the regulation

of their stimulating effects, and this is a thing that is quite impossible to do, even after the most extensive experience in their use. In pulmonary consumption, I have never been able to cure, or even relieve a single case by their use, and I have found them, even in many cases, produce so much excitement, as to render it necessary to discontinue their further application. With respect to the comparative merits of these gases, the one has always appeared to me to be possessed of nearly the same effects as the other, both in consumptive cases, and also in cases of chronic inflammation of the air tubes of the lungs.

Although I consider the inhalation of chlorine and iodine not by any means safe, or generally useful, and although I do not consider any other counter-irritant to be at all compared with turpentine in the treatment of pulmonary affections, still I do not wish to be understood as advocating the exclusive use of that application ; and in order to point out its advantages clearly, and at the same time to explain the various auxiliary remedies I frequently employ in conjunction with it, I will now proceed to give a full detail of the various pulmonary affections in which I have found it most beneficial.

Hooping cough is one of those epidemic diseases, from which most children suffer an attack, and that before they have arrived at their tenth year. It is unfortunately on the pulmonary organs more particularly, that this epidemic disease commits its ravages : every person who has watched the progress of hooping cough, must admit that it owes its peculiar characters, in a great measure, to spasmodic action of the muscles of respiration, but more particularly those surrounding the aperture of the larynx. There is, however, in addition to this spasmodic action, almost always a congested state of the mucous lining of the air tubes : this, however, appears always in the first instance, to be greater about the larynx than any other part. Sometimes, instead of what might be denominated a simple congestion of the mucous lining of the air tubes, there is a more active inflammatory state, and this is even occasionally found to

pervade the substance of the lungs. Now in this disease, the danger would appear to be partly owing to spasmodic action, and partly to inflammatory action, the former being most dangerous in very young infants, and the latter in children of all ages, possessing full habits of body. From these circumstances, the physician should keep two things strictly in view in the treatment of hooping cough, the one is the reduction of spasmodic, and the other that of inflammatory action.

I do not know any single remedy in which I could place so much confidence, in the treatment of this disease, as the application of turpentine, owing to its antispasmodic and counter-irritating effects, when used freely to the surface of the chest or throat. In all cases, after the chest or throat has been freely moistened with the turpentine, a piece of flannel should be closely applied, so as to exclude all access of air, and at the same time assist in keeping up the irritation on the surface. When it is deemed necessary to increase the antispasmodic powers of the turpentine, a proper proportion of camphor or laudanum is added. I have seldom found it necessary to repeat these applications more than once or twice a day, unless the symptoms were very urgent, so as to require active counter-irritation. In addition to the turpentine application in the treatment of hooping cough, I invariably detract blood from the chest or region of the larynx, when I find any feverish state, pain of chest, hurried breathing, or any other symptoms indicating inflammatory action in the air tubes or substance of the lungs. In children of full habits, I employ leeches, independently of the pressure of inflammatory symptoms. When leeches are applied in hooping cough, the bleeding should never be promoted by sponging with warm water after they are detached: it is better, instead of sponging in this way, to apply as many leeches as will be equal to take away the quantity of blood required, so that there may be no exposure of the parts after their removal.

By proceeding with the applications I have just stated, and at the same time attending to the state of the bowels, I ge-

nerally find the spasmodic cough, and all the other morbid symptoms which usually accompany the disease I have been considering, subside, even in the worst cases, in the course of eight or ten days. Internal antispasmodics of different kinds, are very generally employed in the treatment of hooping cough. I have tried a very great variety of remedies of this class, and I must candidly confess, that they have, in almost all cases, very much disappointed me; but although I cannot recommend them as being equal in efficacy to the turpentine application, still there are some of them useful auxiliaries to it in the relief of the spasmodic cough. The internal antispasmodics which are most useful, are opium, hemlock, and henbane, in the form of tincture, either given separately, or in combination with each other, and with a suitable proportion of hippo or antimonial wine. At the commencement, and even during the entire progress of hooping cough, it is very common for physicians to order squills in doses sufficient to excite vomiting several times daily; now this, I think, is a most reprehensible plan of proceeding, on account of the great tendency it has to produce, in some instances, and in others to increase, inflammatory congestion of the mucous lining of the air tubes of the lungs, a thing that the practitioner should take particular care to prevent or remove. Those who order squill emetics, generally do so with the view of promoting the discharge of a quantity of the phlegm which usually abounds so much in this complaint. The removal of the phlegm is certainly a thing most desirable, but it should be remembered that this secretion is merely a product of the spasmodic and inflammatory action which I have stated as constituting the complaint, and that it can only be moderated or checked by antiphlogistic and antispasmodic remedies.

When hooping cough is not treated on the antiphlogistic and antispasmodic plans I have so strongly recommended, it sometimes assumes a chronic character, and the child becomes hectic: now when such is the case, change of air is recommended, and in many instances, this is productive of the very

best effects. I am so much alive to the immense advantages of certain states of the atmosphere, to the recovery of children affected with this disease, that I would, if possible, keep them breathing from its very commencement, in tolerably moist air, at a temperature never lower than sixty-six degrees of Fahr^ts thermometer.

Asthma.—This is another disease that is characterized by great spasmodic action of the respiratory muscles, but more particularly those of the air tubes. It is, however, only as far as spasmodic action of the respiratory muscles is concerned, that it bears any resemblance to whooping cough, for as to its exciting causes, general symptoms, progress, and termination, it is totally dissimilar to the last mentioned disease. In asthma, the mucous lining of the air tubes of the lungs is sometimes perfectly healthy, and at other times it is in a diseased condition, or at least it possesses, in some individuals, a strong predisposition to assume a diseased action, on the slightest application of cold to the surface of the body, or the least change in the temperature of the atmosphere; and hence, sometimes, those paroxysms of difficult breathing which constitute the asthmatic attack, are accompanied with bronchitis, either in a lesser or greater degree. It would appear, therefore, from these circumstances, that in the treatment of asthma, as far as the paroxysm is concerned, sometimes antispasmodic remedies alone will be sufficient, and at other times that these must be combined with such as are possessed of antiphlogistic powers.

I have tried freely antispasmodics of the most powerful kind, in this disease, as internal remedies, and from laudanum, which is one of the most powerful of them, I have even been frequently unable to procure any immediate relief from the difficulty of breathing. And with regard to diffusible stimulants, either alone or combined with laudanum, I have in many instances been little more successful; but such has not been the case when I have employed to the surface of the chest, the turpentine application, until it has produced considerable inflam-

mation. In order to excite sudden inflammation on the surface of the chest with this application, it should be used as hot as it can be borne, and by means of flannel, I have in very few cases failed to check an asthmatic paroxysm by the application I have just stated, in one or at most two hours. I do consider it, therefore, after numerous trials, by far the best antispasmodic that the physician can make use of in the disease in question. When there is either temporary or permanent disease of the mucous lining of the air tubes of the lungs, in addition to spasmodic action of the internal respiratory muscles, as is the case in what is denominated humoral asthma, I would employ once or twice every day, unless too much external inflammation were produced, the turpentine application to the chest, and at the same time, if the disease of the mucous lining of the air tubes were of the inflammatory kind, I would apply leeches as an auxiliary. I wish it to be distinctly understood, that I would in the treatment of spasmodic asthma, either of the purely nervous or humoral kind, use occasionally other remedies in addition to those I have just recommended, for it is of all other diseases, one that would require nearly all the resources of the physician.

An attack of asthma is generally produced by a disordered state of the system, of greater or less duration, so that there is time to apply the turpentine freely to the chest, before the spasmodic breathing commences. When the turpentine is employed for the purpose of preventing the occurrence of a paroxysm of asthma, it should be applied to the entire chest by means of flannel, and at as high a temperature as it can be borne by the patient, for unless there is very extensive redness and smarting produced, it will not answer the purpose.

Croup is an inflammatory affection of the air tubes of the lungs, extending in most cases from the larynx to the extreme ramifications of the bronchiæ. It is the mucous lining that is more particularly affected in this complaint, but there is also very considerable spasmodic action of the internal res-

piratory muscles, so that it should be always considered as a disease partly consisting of acute inflammation, and partly of spasmodic action. This is a disease peculiar to infancy and childhood, and like many other maladies of these periods of life, it runs its course with great rapidity, and very frequently to a fatal termination. It requires, therefore, that whatever remedies are employed for its treatment, should be possessed of considerable activity, both for the relief of inflammation and spasmodic actions. The plan I usually follow, in the treatment of croup, is to detract a quantity of blood as soon as possible after its commencement, the quantity of blood always to be in proportion to the state of the system and the violence of the disease. When children are very young, or not possessed of very strong constitutions, I would prefer the detraction of blood from the chest or throat by means of leeches, and when children are from eight to ten years of age, I would employ the lancet. After the free detraction of blood, the next step is the production of counter-irritation over the chest and neck by means of the turpentine application. When the turpentine has produced a considerable degree of cutaneous inflammation, I generally find the dry barking cough considerably relieved, and by a little further perseverance, all the morbid symptoms subside. Besides this application and the blood-letting, I invariably, at as early a stage as possible, establish perspiration, which I keep up freely, at the same time that I employ the turpentine to the chest and throat. As a diaphoretic I generally prefer an infusion of hippo, to which I add a small quantity of nitre and henbane. The strength of this infusion must vary according to the age of the child, and the effect that is required. The dose should always be such in the first instance, as to give rise to a considerable deal of nausea, but at the same time, care should be taken not to carry it so far as to excite vomiting. In the treatment of all acute affections of the respiratory organs, sweating is a remedy of the greatest importance. It requires, however, to be kept up for several hours at a time,

in order that it may be useful, and I do not know any one disease of this class, in which its curative powers are greater, if carried a sufficient length, than that of inflammatory croup. The salutary effects of sweating remedies are seldom or ever obtained by the common plan of conducting that process; for instead of keeping the skin moist for one or two hours, as is usually the case, it should be kept freely moist for fifteen or twenty hours. Bleeding, early counter-irritation with turpentine, and free sweating, are the remedies on which I would almost exclusively rely in the treatment of croup, not forgetting, at the same time, the necessity of keeping the bowels free with cooling laxatives. By some practitioners calomel is thought to possess wonderful powers, when given to a considerable extent in croup, I have used it in many cases of this complaint to a very great extent, and again I have used it in smaller quantities, and from the numerous trials I have made of its curative powers, I do not think it is a remedy that can be at all depended upon, no matter in what dose it is given. I could therefore wish, that the use of this medicine were dispensed with altogether in the treatment of inflammatory croup.

There is no acute affection of the respiratory organs more liable to suffer a relapse, than the one I have just been considering, so that it is necessary during convalescence to guard against any exposure to cold. In order to prevent any bad effects from exposure to cold, I would advise during the continuance of the acute symptoms, and even for some weeks after these have subsided, that the patient be kept constantly in an atmosphere such as I have stated to be most useful in whooping cough.

Bronchitis.—This is a morbid affection of the respiratory organs, of still more frequent occurrence than any of these I have hitherto considered, and as it is produced by atmospheric changes, particularly from heat to cold, it merits, in a variable, damp climate such as this, the very particular attention of the medical practitioner. In Belfast I am convinced that colds

prevail more than all other pulmonary affections, and as they are frequently complicated, in this and all other places where they prevail to a great extent, with many other morbid conditions of the system, they are the immediate cause of death in far more cases than is generally supposed. Bronchitis, being a disease that attacks at all ages, and even in the most opposite conditions of the system, requires far more attention than almost any other disease of the respiratory organs, as far as treatment is concerned. Its progress in infancy, childhood, manhood, and old age, are all different, and it therefore requires, at each of these particular periods of life, remedies in some degree different from those that would be suitable at any other period. All the eruptive fevers are accompanied with a bronchial affection, and this is in many instances of a very dangerous kind; but that which is so generally complicated with continued fever in the winter season, is productive in very many instances of the most disastrous consequences. This, however, is more particularly the case in those who are advanced in life, or who are possessed of delicate constitutions. I might safely say, that the one-half of those who die of fever in the Belfast hospital, being advanced above thirty-three years of age, die from asphyxia, in consequence of disease of the mucous lining of the air tubes of the lungs. Although bronchitis, complicated with fever, is not so dangerous in manhood or childhood as in old age, still at the two former periods of life, it is not by any means to be considered as a trifling morbid affection, when complicated with that disease. I fear it is too much the case in the treatment of continued fever, to look upon bronchial disease as a secondary affection of no great consequence. Such, however, should not be the case, for at any period of life it is one of the most frequent, insidious, and dangerous complications of fever with which I am acquainted. This is the case particularly during the winter and spring months.

In the treatment of bronchitis occurring at different ages, and in different conditions of the system, I have found no single

plan of treatment at all to be compared with that of counter-irritation of the surface of the chest, by means of turpentine. The antispasmodic powers of this application are of the greatest consequence, when the respiratory muscles are in a state of irritation, and such is the case in the disease of the mucous membrane I am just considering, to a very great extent. It is, besides, an application that as I have already stated, is not liable either to increase the bronchial inflammation, or the accompanying symptomatic fever, as is too often the case with blisters and other stimulating applications, so generally applied to the chest in cases of bronchitis. In the treatment of acute bronchitis occurring in youth or middle age, and not accompanied with continued fever, I would advise, next to the detraction of blood, free counter-irritation of the surface of the chest by means of flannel well moistened with turpentine, and at the same time copious sweating for several hours. In slight attacks of cold I would never think of bleeding, but trust simply to the turpentine application to the chest, free perspiration, and keeping the bowels free by means of some cooling laxative.

In the treatment of acute bronchitis in children, there should never be a large quantity of blood abstracted either by the lancet or leeches, even in the most severe cases, on account of the sudden and dangerous relaxation it is liable to produce in such patients. The safest and most successful plan of treating bronchitis in children, consists in early and moderate bleeding, early counter-irritation with turpentine, and early and free perspiration, together with the use of the mildest laxatives.

When acute bronchitis occurs in old age, the application of turpentine to the chest so as to excite cutaneous inflammation, together with mild diaphoretics and gentle laxatives, will, at an early period of the disease, afford by far the best chance of success. Blood-letting is a remedy that I would not at all recommend, and it is, in my opinion, in most cases very dangerous, when inflammatory action of the mucous lining of the air

tubes of the lungs occurs in a relaxed state of the system, which is almost always the case in persons far advanced in life.

The acute bronchitis which occurs so frequently in this town, in connexion with continued fever, should be treated in almost all cases without the loss of blood, either by means of leeches or the lancet. I do not know how the loss of blood is borne for the relief of acute inflammation of the mucous lining of the lungs in other places, but in this I am quite satisfied, that it is a dangerous practice in nine cases out of ten in continued fever. The great danger from the loss of blood in bronchitis occurring in old age and in continued fever, is the production of relaxation of the system to such a degree, as to give rise to asphyxia, from the accumulation of mucus in the air tubes. I would, therefore, in almost every case of bronchitis, accompanied with continued fever, overcome the inflammatory action by the early and free application of turpentine to the chest. The use of sweating remedies, so valuable in the general treatment of acute bronchial inflammation, is of somewhat doubtful efficacy in the treatment of that complicated with continued fever; and when bronchitis is accompanied with the last mentioned disease, I would treat it in the great majority of cases by the turpentine application alone. It is necessary for medical men who take charge of patients labouring under continued fever, to be most attentive to the state of every internal organ of the body, and there is no part that requires more attention than that of the mucous lining of the air tubes of the lungs. As soon as I ascertain the existence of disease in the mucous lining of the lungs of persons labouring under fever, I cause the chest to be well sponged with turpentine, and to be afterwards closely covered with flannel. The extent to which I carry the turpentine application varies according to the amount of diseased action to be overcome. In severe cases of inflammation, I would repeat the sponging with the turpentine every second or third hour, and in more mild cases I would not, in all probability, repeat it more than

three or four times a day. In the treatment of chronic bronchitis, I find the free use of turpentine to the surface of the chest, generally more decidedly useful, than any other external application, or any internal remedy with which I am acquainted, and it should, at least in that disease, always be considered a necessary part of the curative means that are employed.

From what has been said respecting the advantages of turpentine as an antispasmodic and counter-irritant in bronchitis, I need not dwell at any length on its employment in the treatment of pneumonia and pleuritis. In the last mentioned diseases, I employ it as a counter-irritant from the very commencement of the inflammatory action, and with the very best effects. When it is desirable to excite by the turpentine application to the chest sudden cutaneous inflammation, it is used at as high a temperature as the patient can bear it, and by means of flannel. The plan I follow in most cases in the treatment of pneumonia and pleuritis, when the person is of a strong constitution, is to detract blood freely at as early a stage as possible, and at the same time excite sudden and extensive inflammation on the surface of the chest by means of warm turpentine. The only other remedies I use in addition to these are diaphoretics and cooling laxatives.

Before I conclude this paper, I cannot omit making a few observations on the treatment of pulmonary consumption, which is a disease of very frequent occurrence, and at the same time, one that is less easily managed than any of these diseases of the pulmonary organs I have hitherto considered. The number of cases of pulmonary consumption constantly under my care in the Belfast hospital, is very great, so that I have an extensive opportunity of treating it at every stage. It is precisely the same with consumption, as it is with all other diseases of the pulmonary organs, the physician can do most good, when he is consulted at an early stage, and by the assistance of the stethoscope, he will be able, in the great majority of cases, to detect the presence of tubercles in the lungs, before

they have made much progress. There was a time when the cough, which is usually produced by the presence of tubercles in the lungs, at a very early period of their growth, was, in most instances, attributed to bronchial disease, and hence its real cause was not, in all probability, discovered, until very extensive disorganization of the pulmonary substance had taken place.

In the treatment of pulmonary consumption at an early stage, the physician has two important objects to fulfil, by whatever curative means he may employ ; the one is to promote the absorption of the tubercles, and the other is to retard their growth as much as possible. I do not know any thing so useful for the purpose of promoting the absorption of tubercles, or retarding their growth, as frequent small bleedings, and counter-irritation to the chest by means of turpentine. I prefer the application of a small number of leeches to the chest, to any other mode of detracting blood : the number, in persons of full habit, should never exceed eight at a time, and the bleeding from the bites should never be promoted by sponging with warm water. The best way of proceeding, after the leeches are detached, is to cover the bites with adhesive plaster, and immediately afterwards, apply over the entire chest, a piece of flannel, well moistened with turpentine. In young persons of pretty full habit, six or eight leeches may be applied to the chest twice a week, and in persons of rather delicate constitution, the same number may in general be applied with advantage once a week. The best time to apply the turpentine to the chest in a general way, is at bed time, and one good application at that time, will generally be quite sufficient to keep a slight, but constant irritation on the surface.

By proceeding with the bleeding and application of the turpentine in this way, I have, in several cases, when I had every reason to believe that tubercles were in considerable number in the lungs, been able to remove, completely, the symptoms of pulmonary disease ; and again I have been able, by prosecuting

the same plan with steadiness, to retard, in many cases, the progress of the disease.

In all cases where there are either tubercles present in the lungs, or a strong tendency to their development, the greatest advantage is gained by defending the surface of the chest from cold during the winter season. The chest may be defended from cold in various ways, but I think there is nothing that answers the purpose better, than a very thin layer of cotton wool, fixed in such a way, as not to be liable to shift by the different motions of the body. The application of blisters to the chest, is in very general use at the commencement of pulmonary consumption, for the purpose of affording relief to the pain of chest, cough, and other early symptoms. I am convinced, however, that instead of affording relief, these applications, in very many cases, hurry on the disease to a fatal termination, owing to their excessive irritation, favouring, instead of retarding the secretion of the tubercles. Besides, blisters, if frequently repeated, have the effect, in most cases, of producing great general debility, a thing that should be avoided as far as possible, in every stage of the disease. The debility, and excessive irritating effects produced by the protracted use of tartar emetic to the chest, renders it also an objectionable remedy. There is one stimulating application, besides the turpentine, that I have used to a very considerable extent, in the treatment of the first stage of pulmonary consumption ; it is iodine ointment. This application, however, as well as that of the turpentine, must be attended with leeching. With regard to the use of internal remedies in the first stage of pulmonary consumption, I know none on which I could place more confidence. When the cough is very troublesome, however, the antispasmodic power of the turpentine may be assisted by hemlock, hyosciamus, and hippo, combined together, and given in proper doses.

It is certainly necessary at the commencement, and even during the entire progress of pulmonary consumption, to attend to the food, drink, and above all, to the keeping the body in

an equable, and at the same time, moderately high temperature. These, however, are only auxiliary to the plan of treatment I have so strongly recommended.

With respect to the advanced stages of consumption, I need say nothing, as, after the progress of softening has commenced in the tubercles, the physician can scarcely palliate the various distressing symptoms. There is, however, in the progress of consumption, an affection of very frequent occurrence, that merits a little attention, it is pleuritis. The pleura, when inflamed, produces most acute pain ; and when attacked in the last stage of consumption, has a most distressing effect. Now for the relief of pleuritis, under such circumstances, the turpentine application will be found most valuable, on account of the sudden relief it affords.

I intended, at the commencement of this paper, to illustrate the advantages of turpentine in the treatment of pulmonary affections, by a number of cases ; but I found, before I had proceeded far, that such a plan would have rendered it far too voluminous.

EXPLANATION OF PLATE.

This plate exhibits an accurate representation of the calculi removed by Dr. G. O'Brien. (See Case II. p. 8, of this Number.)

Fig. I. Represents the calculus first extracted, and which broke before being removed. The tooth is seen with the fang turned upwards, the crown being fixed in the calculus: the appearance of the enamel is quite distinct. The fissure beneath marks the commencement of the fracture, where the stone broke.

Fig. II. The calculus subsequently obtained. The superior extremity exhibits the fang of the tooth, protruding, as it was seen, after some of the matter had been scraped away.

Fig. III. A section of this calculus, shewing the situation, and the several parts of the tooth within. The fang, crown, and enamel, are distinctly marked; with the canal, containing a substance, that appears to be the remains of the nerve.

These representations are of the exact size of the originals.

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Fig 1



Fig 2.

Fig 3.

BIBLIOGRAPHIC NOTICES.

Observations on Obstetric Auscultation, with an Analysis of the Evidences of Pregnancy, and an Inquiry into the Proofs of the Life and Death of the Fœtus in Utero. By **EVORY KENNEDY, M.D.**, Licentiate of the King and Queen's College of Physicians in Ireland, Lecturer on Midwifery and the Diseases of Women and Children, at the Richmond Hospital School, and late Assistant to the Dublin Lying-in Hospital.

(Continued from Vol. IV. p. 265.)

WE now come to a most important part of Dr. Kennedy's work, the application of auscultation, as one of the evidences of pregnancy, and of the means of ascertaining the life or death of the fœtus in utero. In some of the most difficult and doubtful crises which occur in midwifery, a knowledge of the life or death of the fœtus may often save the life of the child or mother, and will always materially influence, and to a great degree guide, the practitioner in the performance of one of the most revolting operations in medicine.

The discovery of auscultation, as a means of diagnosis in suspected pregnancy, is due to Maier of Geneva, who heard the action of the foetal heart, by applying his ear to the abdomen of a pregnant woman. Kergaradec put this discovery into practical application, and detected the placental souffle; and the attention of the profession in this country was, we believe, first called to the value of auscultation in doubtful pregnancy, by two papers, published in the Medical Transactions, and Dublin Hospital Reports; the first by Dr. Ferguson, Professor of Practice of Medicine to the Apothecaries' Hall of Ireland, the second by Dr. Kennedy, the author of the work before us.

Dr. Kennedy, in combating the objections to the use of the stethoscope, notices a German writer, Siebold, who asserted, that notwithstanding all the pains he took, he was unable to discover any sound with the "hearing trumpet," as he calls the stethoscope. Siebold, however, admits that he could easily de-

tect the required sounds with the naked ear, while Dr. Johnson, of Edinburgh, in the ninth number of this Journal, speaking for himself, and, as he states, authorized to the same purport by others, assures us he could never hear the foetal heart, and modestly says, that he does not believe the assertion of a respectable member of the profession, as to *his* ever having heard it. We should like, as a curiosity, to have a statistical table of the ages of the disbelievers in auscultation. We would venture a wager, that as in the instance of the sceptics on Harvey's discovery of the circulation, their ages would average above 40, unless, perhaps, with an exception of insuperable ignorance or self-conceit that scorns to learn. Within the circle of our own observation, we have observed men holding a respectable rank in the profession, who first publicly sneered at what they called the quackery of the stethoscope, then prophesied that its use as a toy would not outlive a year, next became silent on the subject, and now, with more cunning than honesty, carry the instrument about with them, pretending to understand its application, and knowing as much of its use as a state trumpeter in one of our city pageantries, knows of the use of the instrument of music which he idly carries as part of the show. When an individual comes forward, and stating himself as speaking the sentiments of a number, says that the persons on whose part he speaks could not believe that "the foetal heart is audible before birth," we have only one of these alternatives to offer, either, 1st, that the individuals on whose part he speaks, are all deaf, forming a party brought together by a freak of accidents, like the party of dervishes in the Arabian Tales all blind of an eye, or 2dly, that having the possession of the faculty of hearing, they will not exercise it, and there are none so deaf as those that *will not* hear; or lastly, that all those in France, Germany, Italy, and our own countries, who say they can hear and have heard the foetal heart, are all linked together to uphold an imposture. The last supposition is too monstrous to be entertained for a moment: the first, Dr. Johnson and his supporters will, from their self-esteem, at once repudiate. In possession of the second, the only remaining alternative, we leave them in the full enjoyment of all their self-gratulations.

The placental souffle, or sound produced in the neighbourhood of the placenta, is the first sound noticed.

"If we examine, either with the naked ear or the stethoscope, the abdomen of the pregnant woman, we shall (provided the pregnancy be sufficiently advanced) observe a peculiar blowing or hissing sound. This sound is to be met with in almost every case, and is

observed at different parts of the uterine tumour. It does not always exhibit the same characters ; yet these are sufficiently striking to render it recognizable in almost every case. It assumes the different varieties which Laennec describes under the term *bellows' sound*, namely, the bellows' sound, properly so called, likened by that author to the continuous murmur, similar to that of the sea, familiarly exemplified by the application of a large shell to the ear ; the rasping or sawing sound, which is occasionally found so exactly imitated as to lead the listener to imagine an artisan at work quite close to him ; and the musical or hissing sound, so well described by the same author. A sound, resembling the cooing of a dove, is sometimes observable, but this is comparatively rare. A more frequent peculiarity to be noticed, is, a strange drone resembling that of a bagpipe accompanying the sound, but yet without interfering with it. The most constant form we meet with, however, is a combination of the bellows or sawing with the hissing sound, commencing with one of the former, and terminating with the latter ; and this is in general so protracted, that the last *souffle* is audible when the subsequent one commences.

“ These sounds are, from the distention of the uterus, and consequent facility of examination, easily detected in advanced pregnancy ; and although not so loud or sonorous in the earlier stages, yet to the practised ear they become equally distinct. None of the above mentioned varieties are peculiar to particular stages of pregnancy, being detected indifferently in them all. The extent of surface over which the sound is observable, varies much according to circumstances ; in some it is confined to a small circumscribed spot, in others it is audible over a greater surface, perhaps two or three inches square ; and in a few it is to be met with over the greater part of the uterine tumour, although there is in many cases one spot in particular, perhaps not larger than the end of the instrument, where the sound is vastly more distinct and sonorous than elsewhere. The *souffle* is most frequently found in the lateral and inferior parts of the uterus, but it may have its seat in any part of it ; and it must be added, that cases will occur, although, if proper precautions be had recourse to, very rarely, where we shall not be able to detect it. The cause producing the sounds in question requires explanation, with a view to which, we must briefly inquire into the nature of the vascular structure and circulation of the uterus.”

Dr. Kennedy examines the structure of the uterus where the placenta is attached, to explain the production of the *souffle*. He observes, that the portion of the uterus to which the placenta is attached, is a circumscribed portion, changing abruptly from the remaining texture of the uterus, and consisting of “ a congeries of vessels, tortuous, ramifying, and expanding into cells or sinuses, whilst the remaining part exhibits the parenchymatous structure of this organ, with merely occasional vessels interspersed.”

After an examination of the facts observed relative to the placental souffle, and a repetition of De Haüs's opinion, that the sound is in the aorta or iliac arteries, our author states his own conclusion, that the souffle depends on the transmission of blood through the arterial tubes of the uterus.

The immediate cause of the production of souffle is next discussed, and Dr. Kennedy adopts the explanation of its production offered by Dr. Corrigan.

“ The manner in which the *souffle*, when situated in the other arteries of the body, is produced, does not as yet appear to have been quite agreed upon. Laennec, after discussing the subject at considerable length, arrives at the conclusion that it depends upon spasm. Dr. Corrigan ascribes the phenomenon to the currents produced by the passage of blood from a narrow orifice into a wider tube. It certainly appears more reasonable to explain what we may, with propriety, at least in the instance of placental *souffle*, term a constant phenomenon, upon the principle of fixed physical causes, than that of spasm, which we can only term a casual and changing vital operation; but besides this, Dr. Corrigan's views would rather appear to be corroborated by the phenomenon in question. Let us bear in mind the difference that exists between the virgin and the pregnant uterus. That, in the latter, a new system of vessels becomes rapidly developed, capable of furnishing with the means of growth a new being, and the appendages peculiar to it. The organ which heretofore received merely a sufficiency of blood to sustain its own vitality, and admit of a slight periodic discharge, in consequence of the alteration in size which has taken place, and as well the comparative increase in the vessels already existing as the development of new ones, now receives a quantity of blood, bearing a very large proportion to that circulating in the rest of the system, while the great vessels supplying this organ do not appear sensibly altered in size. With this view of the circulation we can find no difficulty in admitting, that between this system of vessels and the main trunks supplying it, a disproportion in calibre exists. If we take into consideration with this the peculiar distribution of the vessels of the uterus at the placental part, and the free anastomosis which takes place here and in the lateral parts, the vessels meeting and converging there, and that a comparatively small stream is now rushing from one or two arteries into an extensive system of tubes, the aggregate calibre of which is infinitely greater than that of the vessel or vessels from which it flows; the inference of Dr. Corrigan with regard to *bruit de soufflet* generally, may with every justice be drawn, namely, ‘ that the motion (of the blood) is that of a current; the sides of the arteries, instead of being acted upon by a body of fluid moving forward almost as a solid mass, receive the impulse of a stream whose particles are in motion with different degrees of velo-

city.* The existence of the sound here, beyond the point where the artery is comparatively narrow, tends, as in Dr. Corrigan's experiments, to prove its dependence upon the above causes."

There are some very useful directions given on the means of avoiding mistakes, which might be made by the tyro in the use of the stethoscope, when seeking for the placental *souffle*. For these we must refer to the work. It is scarcely necessary to say, that Dr. Kennedy relies very much on the placental *souffle* as an evidence of pregnancy.

"The co-existence of the foetal pulsation with the *souffle*, of course decides the question; but with regard to the latter, taken separately, we shall even go so far as to say, that were all the symptoms of pregnancy absent, and this sound could be distinctly perceptible, we should at least withhold our opinion until a sufficient time had elapsed to place the matter beyond a doubt."

And two very convincing cases are given in support of this opinion.

The placental *souffle* may be detected at a very early period of pregnancy.

"We have not succeeded in detecting the placental sound in any case until after the second month from impregnation; but have frequently done so in the tenth, eleventh, and twelfth weeks.

"August 15th, 1829. A woman named Devereux, who had been under my care in labour eighteen months before, called to consult me for a slight attack of pneumonia. She mentioned that her menses had not appeared for the last two months: I therefore examined her with the stethoscope, and detected clearly the placental *souffle*, although no uterine tumour was observable. Dr. Collins, who also examined her, expressed his astonishment at its distinctness at that early period. I gave this woman reason to suppose it possible that she was pregnant, of which she had not the slightest anticipation. However, the accuracy of the diagnosis was attested by her coming into hospital on the 7th of March, 1830, in labour, and being delivered of a living child the day following, exactly twenty-nine weeks from the period at which we had examined her."

In concluding his observations on the placental *souffle*, he observes judiciously as to the reasons for demanding confidence in it.

"Considering the *souffle* as an evidence of pregnancy, in an abstract point of view, we should be inclined to inquire what ought more strongly to verify the opinion we arrive at on this subject, or

* Lancet, vol. ii. N. S. p. 1.

afford us more assistance in our investigation, than a phenomenon produced by the passage of blood through the vessels constituting the connexion between the parent and the embryo ; on the supply of vital fluid, through which, however mysterious that connexion may appear, the actual existence of the latter as an embryo, its development as a foetus, and maturity as a child, depend ?”

The next auscultatory sign taken up is the pulsation of the heart of the foetus.

“ In exploring with the stethoscope the abdomen of a woman at the full period of pregnancy, whose child is alive, we shall detect over a surface, more or less extensive according to the position of the child, and disposition of the foetal and maternal organs, a pulsatory sound generally much more rapid than the pulse of the mother, and exhibiting the characteristic marks of a distinct and independent circulation.”

The pulsations of the foetal heart are about 130 or 140, but are considerably influenced by the state of the circulation in the mother, by hemorrhage from the maternal vessels, by the muscular efforts of the foetus, and by the action of the uterus in labour. Some interesting cases are related, illustrative of these points, and very full descriptions are given as to the circumstances influencing the position of the foetal heart, and as to the rules for discovering its pulsations.

The position of the foetal heart is never discovered so early as the placental souffle ; the latter is occasionally detected as early as the tenth week, while the foetal heart can only with difficulty, and by an experienced stethoscopist, be discovered before the period of quickening. Those who, after a perusal of the following case, can be content to remain ignorant of auscultation, deserve to remain for ever in the bliss of ignorance.

“ The author is indebted for the following case to his friend Dr. Byrne, Physician to the University. ‘ Mary M., ætat. thirty-four, married, of a pale, leucophlegmatic appearance, the mother of three children, of bad health for the last nine months ; had no child for the last three years ; catamenia irregular for the same period, both as to quantity and time of appearance. Has been much distressed in mind of late, by a tumor rising out of the pelvis into the hypogastrium, as she cannot satisfy herself with regard to its nature. The physician under whose care she was, had an accoucheur to examine her *per vaginam*, before my seeing her ; who pronounced the case to be one of a tumor, about the size of a clenched fist, growing from the superior fundus of the uterus, and involving the fallopian tubes, and ovaries : he further condemned it as *cancerous*, from a supposed nodulated feel, when examined through the parietes of the abdomen. The treatment usual in cases of cancer had been resorted to, but

without any good effect. On May 29th, 1830, I examined her, being requested to do so, in order to make myself acquainted with the feel of cancerous tumors growing in this situation. Her present symptoms are, occasional sickness of stomach, pains about the loins, appetite bad, tongue clean, bowels free, pulse quick, weak, and small: the catamenia have been suspended for five or six months. The tumor is soft, elastic, and circumscribed, without pain on pressure, and occupying the pelvis and lower part of the abdomen. I can find no nodules on relaxing the abdominal muscles in the usual way; but ascertained that when these were in a contracted state, as in the sitting posture, such a sensation was communicated to the hand in a slight degree. On applying the stethoscope immediately below the umbilicus, I heard a masked murmur, very different from the borborygmus of the intestines: on changing to the right side, on the same plane, the placental murmur was tolerably distinct, but this sound singly was not sufficient to make me pronounce on the nature of the *tumour*. On removing the instrument to the opposite side, I heard the *fœtal heart* pulsating one hundred and forty in the minute, whilst the mother's pulse was only ninety. This I pointed out to an accoucheur present, who had previously examined her, without coming to any satisfactory conclusion. We therefore, without hesitation, informed her that she was pregnant; on which glad tidings I have seldom seen more joy depicted in the countenance of any individual. This poor woman was delivered of a healthy child about two months afterwards.' "

There is, obviously, a great difference in the value of the pulsation of the foetal heart, according as positive or negative evidence may be sought for; its presence is at once indicative both of the existence of pregnancy, and of the life of the foetus, but its absence, unless under circumstances which are fully considered by our author, is not conclusive in the opposite way.

Compound pregnancy, complicated pregnancy, pseudo-pregnancy, nervous or ideal pseudo-pregnancy, and premeditated or simulated pseudo-pregnancy, are discussed, and a mass of information and instructive cases given under each head. We must, however, refer our readers to the work for the information required, and we pass on to the chapter containing "the Inquiry into the Proofs of the Life and Death of the Foetus in Utero." This chapter is ushered in with a page or two of flourishing sympathy on the "common fate of mankind," and a conclusion is logically arrived at, that "a foetus may die at any time:" we did not require to be told that, and where we have the long and rugged road of science to travel, the less of the mud of mawkish sentimentality we have to wade through, the better.

It is scarcely requisite to tell our readers how necessary it

often is, to be able to tell confidently, whether the foetus be alive. We cannot easily forget the delight with which a mother heard the assurance of the life of the child she carried, after the occurrence of an accident which she feared had destroyed it. Our author adduces a number of instances to shew how necessary such knowledge is, both during pregnancy and the progress of labour; its importance in medico-legal questions is self-evident. The usual symptoms supposed to indicate the death of the foetus, whether furnished by the mother or foetus, the true value of these symptoms and their insufficiency are shewn with a masterly hand, and the following cases given in illustration.

“ June 3rd, 1830, I was requested by Dr. Hutton to visit a young woman named Lock, who was advanced in pregnancy, and supposed her child dead. She had been married on the 9th of November preceding, and had not since menstruated. She had suffered from morning sickness, and her abdomen had been gradually enlarging, until within the last three weeks, when it became, as she stated, flaccid, and something weighty was to be felt falling from side to side in it. She had never either quickened, or felt the motions of the child; and now complained much of a feeling of coldness in the abdomen, with general debility, and as she expressed it, heart sickness. Her breasts appeared small, scarcely at all distended, and devoid of milk; but she stated them to have been larger and fuller until latterly. Under these circumstances, the patient and her mother naturally concluded the child to be dead. On applying the hand to the abdomen, no motion was to be felt, but the stethoscopic evidences indicated that the child was alive, a fact which, (to the great astonishment of the patient and her friends), was attested by the birth of a living child, within six weeks from the period of examination. Several similar cases could be adduced, as well as cases in which females supposed they experienced the motions of the child, and asserted, that it exhibited the usual evidences of its vitality, at the very moment there existed undoubted proofs of its death. A case of the latter kind presented itself in Mrs. ———, who had been three years married, during which time she had had three dead children, with none of which she went beyond the seventh month. This she ascribed to a venereal affection under which her husband was labouring when she married him, and which he communicated to her. She was examined by me on the first of February, 1830, when she stated herself to be about six months and a fortnight pregnant, and exhibited every appearance of carrying a living child: a fact which was corroborated by the stethoscope. She was again seen by me, in company with Dr. Whitestone, on the 12th of February, when she still exhibited all the ordinary appearances of carrying a living child: her breasts were full and distended, and she could press out some milky fluid from them, her general health and spirits were

good, the abdomen was tense and full, nor did she experience any sensation of weight, or falling from side to side in it. She stated, that she was confident the child was alive and well, as she had felt its motion on that morning. These could not, however, be detected by us, neither could the stethoscope furnish us with any evidence of its vitality. This woman was delivered within four and twenty hours of a dead child, the funis of which was quite putrid, and the vessels filled with thickened black blood: the child altogether evincing evident marks of having been dead for a considerable time in the uterus."

Dr. Kennedy now proceeds to examine the value of auscultation, in determining the life or death of the foetus in utero, and cases are given in detail fully bearing out our author in the value he places on it. The negative evidence of auscultation, or the absence of the sound of the foetal heart, can only be of value to those who are well versed in the stethoscope, and our author warns all others to be cautious how they place reliance on it. The caution to be observed before arriving at a conclusion derived from negative evidence, the difficulties that surround it are fairly put forward, and useful observations, with a correction of an error advanced by Laennec, on the authority of Ollivry, relative to the changes produced in the placental *souffle* by the death of the foetus, appended.

"We shall now instance a few of those cases in which the foetus was, by stethoscopic assistance, pronounced dead during pregnancy.

"Feb. 12, 1830. The wife of a coach-maker, about eight months pregnant, called upon me, being alarmed at the supposed death of her child. She had frequently felt its motions up to the fortnight preceding, but not since. Experienced no other symptoms of the child's death. Is in perfect health, her breasts full and tense, Dr. Whitestone assisted me in examining her with the stethoscope, and upon repeated investigation, neither of us could detect the foetal heart, but an abrupt and circumscribed *pulsatile souffle* was observable low down and at the right side. This woman was delivered of a dead, shrivelled, and putrid child, three days after our examining her.

"Sept. 2, 1830. I was called to see Mrs. ——— in Mecklenburgh-street, who was suffering from deranged digestion. Suspecting pregnancy in this case, the stethoscope was applied, when a distinct foetal heart and *souffle* were detected, and to her great astonishment I pronounced her pregnant, a circumstance that she had not the slightest suspicion of. She appeared to be in about the fifth month.

"I did not see her again until the 13th of October, when she sent for me in consequence of a discharge of liquor amnii. About ten days previously she had been very much alarmed from her husband

having met with a serious accident, since which time she had been complaining. Having placed her in bed, the stethoscope was applied, and upon the strictest scrutiny neither foetal heart nor *souffle* could be detected. I, therefore, had little doubt of the child's death. She was delivered in the course of the night of a foetus at about the seventh month, which exhibited all the appearance of having been for some days dead. There was scarcely a drop of blood discharged in this case, the foeto-placental circulation having in all likelihood been obstructed for some time before her confinement.

“ Many similar cases, observed by the author, could be here adduced, but it is deemed more satisfactory to give those that have occurred in the practice of others. Dr. W. Stokes, whose acute stethoscopic discrimination is so well known to the professional public, was requested to furnish the author with the result of his observation on this subject, which he was good enough to do in the following letter :—

“ ‘ 50, York-street, June 29, 1833.

“ ‘ DEAR SIR,

“ ‘ In answer to your inquiries, I beg to inform you, that, on two occasions under my observation, the cessation of the foetal heart was followed by the birth of a dead child. Both these cases occurred in the Meath Hospital, during the last epidemic fever, with which disease both the mothers had been attacked, and under which they were labouring at the time of delivery. I regret much that I did not preserve accurate notes of these cases; but I have a distinct recollection, that in both instances we heard the foetal pulsations for several days, and in one of the patients, in a great variety of positions.

“ ‘ In one case, I think for two days before delivery, the sounds could not be detected, and the woman was delivered of a still-born foetus, apparently between the sixth and seventh month.

“ ‘ In the other case, three days before birth, I gave it as my opinion that the child was dead. This was the case in which we had heard the foetal heart in such a variety of positions. I think four days elapsed between the cessation of the sounds and the delivery, and the child, which was fully eight months old, was of a very dark livid colour. I have no doubt that, with due precaution, the death of the foetus in utero can be often discovered by the use of mediate auscultation.

* * * * *

“ ‘ I remain, dear Sir, your very obedient,

“ ‘ WILLIAM STOKES.’ ”

“ ‘ To Every Kennedy, Esq., M. D.’ ”

Dr. Kennedy proceeds to combat, and successfully, the opinions of those who assert, that it is a matter of no consequence

to ascertain the life or death of the foetus during labour, the rules to regulate practice being derived, according to them, from the symptoms of the mother, and in no way influenced by the state of the foetus. In support of his views, our author quotes Dr. Dewees' opinion.

“In many instances it would be highly important to us, did we with certainty know that the child was dead in utero. It would often abridge the sufferings of the poor woman, and sometimes spare the accoucheur many a deep drawn sigh, or even, perhaps, appease a disturbed conscience ; but this is a matter of great difficulty, as well as oftentimes of great moment, to decide.”

We shall not go through all our author's arguments. It is sufficient to convince any one of the importance of knowing whether the foetus be alive or dead in a case of labour, to remember, that the instrument to be employed, the forceps or crotchet, may be selected on such knowledge. The presumed usual symptoms of the death of the foetus during labour, as the falling of the uterus from side to side, the cessation of the motions of the child, the feelings of the mother, the fetid discharge, the evacuation of meconium, want of pulsation in the fontanelle, or funis, &c., are all examined in turn, and pronounced insufficient evidence : and lastly, our author comes to the examination of the value of auscultation, as an evidence of life or death of the foetus, and to the rules for making use of it as a test.

“For some days before labour setting in, it is a well known fact, that the abdominal tumour descends, the uterus falling more into the axis of the inlet to the pelvis, the head of the child resting, at the commencement of labour, directly at the upper pelvic aperture. At this time the action of the child's heart is to be observed, generally, most distinctly at the side to which the body is placed, and opposite to that where the limbs are to be felt. It is usually somewhat lower down than during pregnancy, and is often observed to spread completely across the lower part of the abdominal tumour, being occasionally detected at the other side.

“When labour sets in, the uterus still descending a little, the pulsation will be lower, and heard over a more or less extensive surface, as the head adapts itself to the different measurements of the pelvis, and the foetus assumes a more or less oblique position. As the labour advances, the pulsation is observed lower, until at length, in some cases, it is perceptible only at a spot immediately over the ramus of the pubis. The head now becoming engaged in the lower strait, the face gets into the hollow of the sacrum, when the back of the child comes gradually in contact with the parietes of the abdomen ; and now we have the pulsation, although it may have been previously confined entirely to one side, generally extending completely across

the pubic, and often heard in each iliac region; where we may observe it even when the head is pressing on the perineum.

“ This is the state of the case in ordinary pregnancies, where the head presents in its most natural manner, namely, with the occiput towards the arch of the pubis, the face being towards the sacrum. When the face is turned towards the pubis, we observe nearly the same phenomena. In the latter stages of labour, the pulsation extends in a similar manner over the pubis, the breast in this case applying itself to the abdominal parietes, as we observed the back to do in the former case. When the face is the most depending, or as it is termed, the presenting part, we cannot in general observe the foetal pulsation so distinctly: at least such was the result in two cases of the kind which we explored. This may depend a good deal on whether it is the mento-sacral, or mento-pubic presentation. In the cases in which we observed it, the chin of the child was towards the sacrum, which caused the body of it to be pressed from the walls of the abdomen against the spine, thus rendering the pulsation less distinct: whether the same circumstance would be observed in the reverse position, is questionable; as in that case, the chest of the child would be pressed against the abdominal parietes, and *from* the spine; thus bringing the child's heart into more immediate contact with the former. In presentations of the vertex, the sound was heard much as when the occiput presented, and generally over rather a greater extent of surface: whether this arose from the position, or that the action of the child's heart is here more laboured, it is difficult to say.

“ From the cases of arm presentation, which have hitherto been submitted to stethoscopic examination, we should not deem ourselves justified in arriving at any general conclusion, as to the position in which the foetal heart should, in such, be audible. In cases of breech presentation, the foetal heart's action is observed higher up, and according to the state of advancement of labour at the time of applying the stethoscope, above or below the umbilicus. During the progress of pregnancy, and when the breech is resting at the superior strait of the pelvis, the pulsation is generally perceptible above the umbilicus, and at the right or left side according to the child's position. *Plate III. Fig. 3*, will serve to illustrate this fact. In most of the preceding cases, the placenta is attached lower down in the uterus, and its *souffle* is in a considerable proportion audible at one or other side, and not unfrequently at both. We have not the heart's action at the ramus of the pubis in breech presentations, as we have in cases where the head presents; although there is occasionally a pulsation to be met there also. When the breech presents, with the thighs turned towards the sacro-iliac symphysis, as is most frequently the case the phenomenon of the foetal pulsation is both more distinct, and more extensive; a circumstance which is easily explained, if we consider for one moment, the position which the foetus then occupies, with regard to the abdomen of the mother. In this position, in proportion as the thighs and belly of the foetus approach more or less to the sacrum

and spine of the mother, will its back come in contact with the abdominal parietes, thus offering a substance, well calculated to conduct the heart's action, and in fact, bringing the heart nearer the surface for our detection. In the position we speak of, however, the pulsation of the foetus is sometimes heard extending from two or three inches above the umbilicus, over the whole of the anterior part of the abdomen, inclining to one or the other side, according to the position of the back of the foetus. In this way it sometimes is to be detected so low as the pubis, and even in the right or left hypogastric region, where we observe the heart's action in cases of head presentation. It is not throughout this space so distinct in its characters, as it is immediately over the part corresponding to the chest of the child, which is in general near the maternal umbilicus, marked in the figures by a circular point. The sound heard below in these cases, may either depend upon the foetal heart's action being conducted, as we have seen it may be, along the back of the child to the inferior part of the abdomen, or upon the pulsation of the umbilical cord, which may be here situated. In foot and knee presentations, the same observations will pretty nearly apply, with regard to the situation of the foetal heart, as in cases where the breech is the most depending part."

The difficulties to be encountered in stethoscopic examination during labour, cannot be easily overcome by the mere beginner in its application.

"We must be prepared for certain difficulties in exploring in cases of labour, in addition to those already enumerated, as meeting us in ordinary cases. The effect of uterine contraction is first to convert the *souffle* into a pulsation, and eventually to suspend it altogether, whilst the contraction continues, allowing it gradually to return whilst it subsides. The action of the uterus, also, generally prevents our hearing the foetal heart. We can, therefore, learn nothing whatever, if we examine during a pain, or while the uterus is in action. The best plan will be to commence immediately after the subsidence of the pain, by which means we shall have the longer time to conduct our examination before the occurrence of the next: however, the mid-time between the termination of the last and the commencement of the subsequent pain, is the period at which we shall experience least interruption.

"The state of the bladder as to distention or contraction, will affect the distinctness of the sounds. The bladder, from pressure upon its neck in the progress of labour, is often prevented emptying itself of its contents, when, the secretion of urine proceeding, this organ becomes gradually more and more distended, and consequently rises up between the uterus and parietes at the anterior part of the abdomen, forming a tumor more or less circumscribed. This generally only renders the foetal heart less distinct, but in some cases it actually prevents its being heard, a circumstance that indicates the

necessity of using the catheter before pronouncing on the child's death."

In this, as in all the preceding parts of the work, the views put forward are supported by cases. We have room for only one.

" *March 28th, 1829.* Peggy Gallagher, ætat. 34, was admitted into hospital in labour of her third child. She had been delivered of her first child with the crotchet, and her second had been dead born, after a very protracted labour, from which she had a bad recovery. On examination, the pelvis appeared contracted and small; the soft parts were found dilated, and membranes ruptured. The head was engaged in the pelvis, where it was locked, but the face had not as yet got into the hollow of the sacrum. The bones of the cranium were very much overlapped, the head seemed to have already suffered from pressure, and there was an elongated tumour upon it. It was reported by the female in attendance on this patient before her admission, that she had been for some days in labour, and that matters had been in their present state for the last fourteen hours. The bladder was very much distended, and the catheter being with some difficulty introduced, about three half pints of deeply coloured urine were drawn off. The stethoscope was now applied, and the foetal pulsation was discovered at the right side, beating 140 in the minute, while the placental sound was heard at a spot immediately over the ramus of the pubis, 84 in the minute, (the rate, of course, of the mother's pulse). After an interval of eight hours, the head was found to have made no advance, but there was considerable heat of the parts. The maternal pulse and souffle had risen to 100 in the minute. Tongue furred and dry, flushing, with much heat of skin, and the urine, which was drawn off in small quantity, was tinged with blood. The foetal heart distinctly perceptible, 150 in the minute. In twelve hours afterwards the head was little, if at all advanced; a copious and extremely fetid discharge flowed from the vagina, which was perceptible throughout the whole ward. No foetal motion had been observed for several hours by patient. The maternal pulse 120, but full. Tongue dry, with much flushing, and complains of debility. On introducing the catheter no water could be drawn off. Under all these circumstances, the probabilities of the patient's getting well of herself were very remote indeed, whilst the symptoms, in the opinion of many, were such as would call for immediate delivery. The forceps here were out of the question, from the size of the pelvis, (the head being as firmly bound as in a vice,) and there had been no want whatever of uterine action. The perforator was, then, the only alternative, and before having recourse to it, the stethoscope was again applied by Dr. Collins and myself. The foetal pulsation was still remarkably distinct, although increased in frequency to about 160. The placental sound was also perceptible. Under these circumstances, it was determined to allow nature to exert her energies for some time longer, watching closely the occur-

rence of any immediately urgent symptom ; and in two hours, this poor woman was, to our infinite satisfaction, naturally delivered of a living girl, who did well. The only inconvenience attending the delay being the slough of a portion of the integuments over the parietal bone of the child, the effect of the protracted pressure.

“ In this case, then, the child was, by the stethoscope, clearly proved to be alive, although the popular symptoms of its death were present ; and the result evinced not only the correctness of this means of diagnosis, but its practical utility in assisting us in cases where the necessity for interference admitted of doubt.”

Dr. Kennedy strongly recommends the application of the stethoscope as the most delicate test to ascertain the presence of the action of the heart in the still-born child. We should rather trust to the naked ear as being more ready of adaptation to the delicate thorax of the new-born infant.

We have now finished our review of Dr. Kennedy's *labour*, (we mean no pun upon the word,) and we are rejoiced that we have deferred the conclusion of our review to this number, for in the interval between the publication of the first part of our review, and this the concluding portion, Dr. Kennedy's publication has undergone the ordeal of criticism in other countries, and has earned for its author the justly deserved and well-merited praise, which we had felt was his due, but which we were chary in bestowing, lest it should be supposed that prejudice or partiality influenced us in favourably noticing one of the few works brought out in our own city ; but after the warm recommendations bestowed on the work by distant periodicals, we feel assured that we may now, without any imputation of partiality or favour, say, that Dr. Kennedy has supplied a want long felt in obstetric medicine, that he has earned the highest praise for the industry and enthusiasm with which he has cultivated auscultation, as applied to Midwifery, and in short that any one presuming to practise midwifery, without an attentive perusal of the discoveries contained in his work, would be even more culpable than the physician who would presume to pronounce on obscure diseases of the chest, without a knowledge of thoracic auscultation.

D. J. C.

Transactions of the Medical and Physical Society of Calcutta. Vol. VI.

THIS volume evinces a degree of talent, industry, and enterprise on the part of our professional brethren in India, that

places them in the foremost rank amongst the contributors to the advancement of medical science. It contains a number of well executed original papers and essays, the majority of which (as might have been expected) are directed to subjects of local interest, but there are still some, to which the attention of practitioners in any clime or any nation may be advantageously directed. Amongst these, one on the treatment of malignant ulcer and hospital gangrene: one on the ligature of the carotid arteries: and one on lithotrity, for this operation is making its way in India, are particularly entitled to notice.

I. Remarks on Malignant Ulcer, and Hospital Gangrene, by J. L. Geddes, Esq. Assistant Surgeon Madras Service.

In a country, the climate of which is unfavourable to the success of surgical operations, and in which the prejudices of the natives render it difficult, and sometimes impossible, to persuade them to submit to the knife, it was scarcely to be expected, that the bold measure of amputation should be resorted to in this frightful and destructive disease with such success, as to induce the unhappy sufferers in many instances to demand it. Yet, this is the lesson to be learned from Mr. Geddes' paper. In a severe visitation of the hospital gangrene at Prince of Wales's Island, in 1827, amputation of the limbs was resorted to in a number of instances, and exactly one half of the patients saved: and the author expresses a conviction, that had he acted in the same manner, and on the same principles, at an earlier period than he did, several lives might have been preserved. In civil life, the occurrence of this disease is, fortunately, very rare; the cleanliness, regularity, and good government of our hospitals, presenting insurmountable obstacles to its approach or its continuance; but in military practice the case may be far otherwise, in consequence of long residence in one place, want of accommodation, difficulty of procuring wholesome provisions, crowded hospitals, or, in short, any of those numerous hardships and privations to which the soldier must occasionally be subjected. It is then to the military surgeon that these observations are more peculiarly directed.

It appears that without any ostensible cause, at least without any cause sufficiently explained, this disease made its appearance in the hospital of the 25th regiment, Madras Native Infantry, in the early part of the year 1827, and soon extended rapidly and destructively. Previously there had been no case of consequence in the hospital, most of the patients suffering from local complaints, the greater proportion of these being itch.

About the middle of February the malignant sore appeared, and evinced its destructive tendency : every itch-spot and slight abrasion of skin, from any accidental cause, soon began to ulcerate and degenerate into a foul phagedenic sore ; and immediately there were about forty patients, the greater number of whom were suffering in an aggravated degree. The sore in its first stage appeared to be entirely confined to the skin and cellular substance ; it spread rapidly through these textures until apparently checked in its progress by the remedies employed ; and when hopes of amendment were most sanguine, suddenly its ravages extended much deeper, and muscle, tendon, and blood-vessel, were rapidly invaded and destroyed. The men's health soon sunk under the influence of the disease. Amputation was had recourse to in some of these latter dreadful cases, not with any sanguine expectation of ultimate success, but with the hope that nature might have resources and afford relief, when thus far aided by art. Operations in this stage were performed only as the last sad and unpleasant alternative, the patients' full consent to the measure being first previously obtained.

“ The greater number of the sores, as before observed, originated from such slight causes as an itch spot, a slight excoriation of cutis, or from an accidental scratch or bruise. In the space of twelve hours the injured spots increased to the size of a dollar, and gradually enlarged : two or three smaller sores coalesced, and formed one of considerable magnitude : these were usually surrounded by a deeply inflamed purple coloured base, very painful on being pressed, and feeling soft and puffy on the application of the fingers : the ultimate extent of the ulcer usually corresponding with this circumscribed appearance. The ulcerative surface varied in different subjects.

“ These were the milder forms of the complaint : when it began to spread and assume a more malignant aspect, the ulcers presented every variety of disorganization. The edges were ragged, elevated, indurated, and the sore penetrated deeply into the substance of the part affected. The inflammation extended far beyond the space originally attacked : œdema of the feet and ankles supervened : large sloughs were formed, and hæmorrhage from the neighbouring vessels took place in a greater or lesser degree : the discharge was excessive and the foetor intolerable. The extent of the sores was, in many instances, very great : there were two or three cases, where, from the tuberosity of the tibia down to the ankle and over the instep, the whole limb was denuded. One case went rapidly through its career to a fatal termination, gangrene almost immediately taking place, and carrying off the patient : several small abscesses that were opened by the lancet, were converted into this formidable ulcer, one in particular, which was a common boil, on the inner part of the right arm, spread so rapidly, as to make me dread exposure and ulceration of the

humeral artery ; this accident, however, did not happen, but after a tedious trial of many different remedies, granulations formed, and the sore became more healthy. During the progress of these cases, the men's sufferings were intense, and the constitutional irritation necessarily great; stout, healthy men were worn down to mere skeletons.

“ During the progressive stages of hospital gangrene, the mental sufferings almost equal the corporeal ones ; a sad and gloomy feeling seems constantly to harass the minds of patients affected by it ; there appears no hope ; there exists a perfect indifference as to their situation or fate, except as far as regards present pain ; *the poor creatures cry aloud for amputation or death.*

“ I have seen no other disease (except cholera) where prostration so rapidly supervenes.”

The circumstances that first led to the performance of amputation do not appear, unless that it was done as a matter of experiment, and having succeeded was persevered in. The fact, however, being established, may lead to important results, as affording encouragement to perform the operation in other forms of spreading and destructive disease, for there is no form of ulceration or of gangrene, let us call it by which name we please, more likely than this one subsequently to attack the stump. At the same time, it may be remarked, that as a remedial measure it never can become very applicable to hospital gangrene, for the following as well as other reasons.

There can be little doubt of the infectious nature of this disease : it runs through an hospital with amazing rapidity, attacking persons at the remotest parts of the ward, or even in different wards nearly at the same time, and this too, without any likelihood of matter being conveyed by means of towels, sponges, &c. from one sore to another. Assuredly, in such a pestilential atmosphere, no sore is more susceptible of disease, than the surface of a stump in a poor debilitated, emaciated creature, a susceptibility acknowledged by our author in the following words : “ To obviate the danger of the stump becoming contaminated with the prevailing affection, we must be careful, *if possible*, to remove our patient to a purer atmosphere, to inculcate the absolute necessity of cleanliness, and to use every precaution that no communication take place between the convalescent and affected ;” and as certainly if sufficient accommodation could be procured to permit of this perfect separation, after the operation had been performed, it would be better to employ it in preventing the necessity of resorting to it at all. The true mode of treating hospital gangrene is, to break up the establishment in which it makes its appearance, to dismiss every patient capable of being removed, nay, more, it is often unsafe

to admit any new patient for weeks, or even months after every known method of purification had been employed. It is, therefore, difficult to understand why any patient should be brought into such a focus of infection, as would convert an itch-spot into a malignant and gangrenous ulcer while there existed a cot, a tent, or any kind of shelter into which he might be removed, after having been subjected to a cruel disease and a dreadful mutilation. On this point, we are furnished with no information. Perhaps from the words "if possible," in the above quotation, Mr. Geddes could not always avail himself of any means of separating the healthy from the infected, and if so, it is easy to explain why only half the amputations were successful. The only matter of surprise is, that even one recovered.

Of the style and manner in which this little paper has been drawn up, we have given sufficient extracts to enable the reader to judge: of the difficulties which the writer may have had to encounter in managing a disease so terrible in its effects, no one, at such a distance, can be competent to form an opinion: but it is impossible not to admire the tone of decision with which he insists on a practice, that in his hands had been successful. "Waiting (he says) for the line of demarcation is only sealing our patient's death warrant, and decision in a case, as it indubitably is, of life or death, must be prompt and final." There are other affections as well as hospital gangrene, in which a delay until this line of separation should be formed, has cost many a patient his life.

II. Ligature of the Carotid Arteries in Epilepsy and some other Diseases, by J. B. Preston, Esq.

Not quite thirty years have elapsed since Sir A. Cooper first tied the carotid artery, and probably he then felt some little fear and trepidation, lest the sudden interruption of such a current of blood might occasion unpleasant, or even fatal results to the delicate organ it was destined to supply. But the march of intellect moves onwards with overwhelming rapidity, and now, both these important vessels are taken up in the same individual, with little interval of time between the operations, not only without anxiety, but in the strong expectation of diminishing cerebral excitement, and removing disease. That such were the pathological views on which these bold operations were undertaken, may be gathered from the following passage which we insert in the author's words:

"If the operation should prove successful, in only a small proportion of cases of epilepsy which are beyond the reach of other reme-

dies, it will be deemed worthy of some estimation; and it might be advisable in most of those cases, where there is evidence of predominant cerebral irritation, or of local plethora of the brain. At the same time when we remember how often epilepsy terminates in paralysis, idiotcy, or apoplexy, and when we observe in many of these cases a succession of symptoms, each of which is more and more distinctly referrible to a morbid condition of the brain, and more destructive of the intellectual powers, we are induced to consider whether it be judicious to allow even recent but very aggravated cases of epilepsy to go on until irremediable organic changes are established, without taking measures permanently and effectually to diminish the circulation of blood through the brain."

As in this country, it will be long before epilepsy shall be considered so purely a surgical disease, as to be resigned at once into the operator's hands, the subject might be dismissed with a single expression of admiration at the coolness and non-chalance with which operations, here deemed so formidable, are undertaken in India. But there is one question connected with this subject, that cannot be allowed to pass unnoticed, although our limits prevent its full discussion. Is epilepsy, or are the diseases analogous to it most frequently connected with arterial excitement, or with venous congestion? And if the latter supposition be true, will not the ligature of the carotid arteries, by cutting off the impulse of the heart from that portion of the circulation, rather tend to aggravate than to palliate the disease? On this point neither of the cases detailed offers any thing satisfactory; from the first nothing is to be learned, because the patient appears to have been lost sight of a month after the second operation: from the second case we gather, that after two years and a half of suffering, little or no material benefit had been obtained. An abstract of them is here subjoined.

CASE 1. Headach and partial Paralysis.—Francis Fullingfan, a robust man, æt. 24, admitted August 10, 1831, had been ill from the preceding April, at first with intermittent fever, and subsequently with continued headach, for which the usual remedies were ineffectual.—Symptoms on admission, constant pain in the head: distortion of the face to the right side: feebleness, and inability to walk: partial loss of motion in the left arm and leg: vision of the left eye greatly impaired: appearance of idiotcy in expression and manner, without the judgment being affected: alvine discharge occasionally involuntary: wandering at night.

After a trial of iodine, of a seton in the neck, and a blister to the head without benefit, the right common carotid was tied on the 2nd September. On the 7th he was free from headach, and improved in strength. On the 27th he walked five miles

which fatigued him very much: the next day he complained of great heat in the head, and impaired vision of the left eye, his head was shaved, and a blister applied between the shoulders. October 3, vision not improved, although the head was quite free: on the 6th he was ordered calomel and opium, which salivated him slightly, but without relief. The left common carotid was tied on the 10th, and on the 11th November he left the hospital: vision continued imperfect, but in every other respect he had entirely recovered his health. He was subsequently discharged on account of the defect of vision.

CASE 2. *Epilepsy and Hemiplegia.* John Parcott, æt. 51, admitted 22nd August, 1831. A short stout man: his neck stiff and short: his head drawn down upon his chest: his eyes full, prominent and staring: had been subject for six years to severe epileptic fits, which came on six or seven times in the month.—Symptoms on admission, complete loss of power of the right side, with great impediment to utterance: the muscles of the face drawn towards the left side: this attack of hemiplegia occurred twenty days previously, after hard drinking. Feet cold: great irritability of temper: the right thumb bent upon itself, and kept down upon the palm of the hand: headach for a long time: intellectual functions not affected. On the day after admission, the right common carotid artery was tied.

The daily reports are tedious, so we shall not repeat them, but on the 26th September, one month and four days after admission, the patient was discharged, although he had experienced seven epileptic fits since the operation. He was, however, re-admitted on the 13th November, and the other carotid tied at once. We shall not follow the reports, but come directly to the result; on the 25th January,—

“ He has had three fits since the 3rd December; they do not render him, even during their continuance, at all insensible. He can scarcely speak; and walks with great difficulty, with the assistance of a stick. He suffers from time to time, from a painful feeling of throbbing and fulness in the head, which is relieved by bleeding. Although a cure has not been accomplished in this case, owing most probably to some serious organic lesion of the brain, still much has been accomplished. He is able to move about with the assistance of a stick, to sit up and enjoy the society of his family. I believe that the operations have prolonged his life: the relief at first afforded by ligature of one carotid artery was very marked, and he is now much better than before he underwent the second operation. His life, I conceive, he holds by a very uncertain tenure; and that it will probably terminate at no very remote period by the bursting of some blood vessel within the brain.”

January 17, 1833, (one year and five months after the cure was undertaken).

“ About three months ago this man had an attack of paralysis agitans, with total loss of speech, but without any impairment of the intellectual functions. These symptoms entirely subsided after he had taken iodine, with purgative medicines, for a month ; and he is now able to walk about with one stick with ease, and speaks with very little impediment.”

So much for tying the carotids as a cure for epilepsy ! But it is a barren field from which something cannot be gleaned, and the mere fact of these two vessels having been tied in two different individuals, worn, wasted, broken down, and epileptic, is most interesting in a physiological point of view. Within a short space of time, the two femoral arteries of the same individual have been tied in one of our own hospitals, (see Mr. Collis's paper in the present number of this Journal), and many can recollect the objections that were raised against the attempt : but when in such a climate as India, and on the very day of a patient's admission, without previous preparation, and almost as a matter of course, such a vessel as the carotid can be tied, and tied successfully, we are almost tempted to ask what we have been about in Europe for so many years, and why is it that we regard such an operation with a respect any higher than Louis did that of bronchotomy, “no more troublesome or dangerous than common venesection.” Our limits will not permit us to notice other papers of interest in this volume. Among them is a report of cases of lithotrity, by Dr. Cusanova, which possesses much merit.

W. H. P.

A Lecture on the Functions of the Lymphatic System. By ROBERT J. GRAVES, M.D., M.R.I.A., King's Professor of the Institutes of Medicine. Hodges and Smith, Dublin. Second Edition, pp. 31.

THE views which are contained in this paper were brought forward by Dr. Graves in the session of 1827 and 1828, a circumstance which we are anxious to notice, as giving to him the merit of a priority in the development of opinions of great interest to the physiologist, and importance to the pathologist. It is interesting to find, that the celebrated Carus of Dresden, in his paper on the Applications of Comparative Anatomy to Physiology, has adopted the same views as those of Dr. Graves, and, indeed, uses almost the same expressions in speaking of the lymphatic system.

“ It deserves particular attention, says Carus, that we meet in the vertebrated animals, a repetition of a vascular system, destined to carry a simple colourless blood, (the lymphatic system,) to which is added another system of vessels of a still higher rank, and destined for the circulation of red blood : this repetition is precisely analogous to the repetition of the ganglionic system of nerves in the higher classes of animals, after the cerebro-spinal nerves have been superadded to that system.”

The views of Dr. Graves were first developed in his lectures on physiology, in the year 1827, while the paper of Carus did not appear until 1828, in which year it was published at Dresden.

Taking as the foundation of his argument, the division of the bodies of the vertebrated animals into red and white tissues ; in other words, into tissues, on the one hand supplied by vessels carrying fibrine and colouring matter, and on the other, by vessels whose blood is essentially albuminous and colourless, Dr. Graves endeavours to shew, that in the white tissues of the red and warm-blooded animals, such as the serous synovial, and cellular membranes, cartilages, &c., there are vessels continuous with the arteries, but conveying only the serous or white blood ; and further, that these tissues are abundantly supplied with vessels for the purpose of returning this white blood, which vessels, according to him, are the lymphatics of former authors. Thus in the circulation of the white parts, the lymphatics are to the white arteries, as in the red parts, the veins are to the arteries carrying red blood.

Many of our readers are aware, that several physiologists, and among them Rudolphi, have denied vascularity to the white structures, holding that the exhalations of these parts are derived from the structures in connexion with them ; in other words, that they are transudations, just as the sweat exudes through the epidermis : the following circumstances are, in our author's opinion, conclusive in disproving the above doctrine.

1st. The appearance of red vessels in white tissues, when in a state of irritation. That these vessels pre-existed in the tissue, is proved by their sudden appearance under these circumstances. Now if we admit their previous existence, it follows that the fluids circulating in them must have been white, for otherwise they would have been visible.

2ndly. The inflammatory vascularity of parts unconnected with any red tissue, as for instance the pseudo-membranes formed between the pleura pulmonalis and costalis, and also the synovial membrane, which is super-imposed on other white tissues—the cartilage and capsular ligament.

These are the principal arguments : others are deduced from the similarity in composition of the cellular and serous exhalations with the serum of the blood ; and also from the experiments of Mayer, who injected solutions of prussiate of potash into the lungs of animals, and in a few hours was able to detect its presence in many of the white tissues. We may remark, however, that the result of this experiment may be in some degree explained by the singular permeability of the living tissue to certain fluids, as demonstrated by Dutrochet and Mitchell.

In support of the opinion, that the lymphatics are the veins of the white parts, the following analogies between them and *veins* commonly so called, may be referred to : 1st, the provision of both with valves ; 2nd, the equable current of fluid in both towards the heart ; 3rd, the contents of both being subjected to the process of aeration, and hence the reason of that portion of the fluid, which had not joined the red veins before, being emptied into them, previous to their arrival at the heart. Fohman has described a large lymphatic trunk conveying lymph to be aerated in the gills of fish, while another trunk conveys venous blood to be aerated in the same organ.

“ This fact, says Dr. Graves, evidently accords as much with the idea of the lymphatics conveying back white blood to be renewed by respiration, and rendered again fit for the nutrition of the white parts, as it is irreconcilable with the commonly received opinion, of their containing the useless *debris* absorbed from the various organs.

“ Many objections, indeed, of great weight, have been urged against the opinion, that the lymphatics convey back the *debris* of all parts of the body, or in other words, serve the purpose of removing by absorption, all that has become useless in the different structures. The fluid they contain is too colourless to be the vehicle of coloured particles, and is too simple and uniform in its composition, to allow us to suppose it to be formed by the union of particles absorbed from structures so various as those of the body.

“ That the lymphatics perform the office of returning the white blood, (by white blood I mean not a fluid absolutely colourless, but a fluid destitute of the peculiar colour of red blood,) is further rendered probable by the fact, that when in disease red blood finds its way into the white capillary arteries of white parts, the lymphatics are found to carry red blood from the inflamed part, and in suppuration, purulent matter has been also found in the lymphatics.”

The fact just mentioned, of the lymphatics carrying red blood was, we believe, first noticed by the celebrated Mascagni in cases both of pulmonary and abdominal hæmorrhage. Among others, Dr. Graves quotes Cruikshank, as describing the same phenomenon. From these facts we should expect,

that, in cases of suppuration, the lymphatics would contain purulent matter; and that such is the fact we have no doubt, though Magendie, in relating a case observed by Dupuytren, and reported by Cruvelhier, attempts to show that the fluid in the lymphatics coming from the suppurated part, was in reality not pus.

On this subject Dr. Graves is fortunate in being able to quote a most convincing case (of fatal psoas abscess) which occurred under his own care, the particulars of which are published in the fifth volume of the Dublin Hospital Reports; from which we shall quote the following account of the post mortem appearances.—“The abscess contained a large quantity of healthy pus. The internal surface was quite smooth, as if it had been lined with serous membrane, and exhibited, towards its infero-posterior portion five or six orifices of the diameter of peas, funnel-shaped, and having their surfaces perfectly continuous with that of the sac. They terminated in organized tubes, which appeared to be lymphatics, for they led to a mass of diseased glands that lay on the brim of the pelvis. Having carefully separated the sac, the lymphatic glands, and the thoracic duct, we found the vessels between the glands and the abscess filled with pus, precisely similar to that of the abscess, while the glands were distended with matter evidently of a similar origin, but changed in its physical properties. In some it was still fluid, but much thicker than in the abscess; in most it was converted into a soft cheesy mass. From these glands ascended a chain of lymphatics, communicating with the thoracic duct, and containing solid matter resembling that of tubercles. The thoracic duct was distended to the size of the middle finger, and felt hard and nodulated. It was found to contain a similar matter, but much harder in consistence, and mixed with a large proportion of a calcareous substance, such as occurs in diseased bronchial glands, &c.”

This case must be considered as decisive of the point. We believe that the appearances observed in this instance are perfectly analogous with what are seen in certain cases of acute and chronic ulcerations of the small intestines. In the first variety we may find the lymphatics proceeding from each patch of ulceration, full of reddish purulent fluid, and the gland in which they terminate containing a quantity of the same. This we have observed in cases of bad gastro-enteric fevers. On the other hand, in chronic ulcerations, nothing is so common as that the situation of the ulcer shall be pointed out by the appearances of the congeries of lymphatics, as seen through the peritoneum; gorged, white, and containing a semi-fluid matter, which is

to be deposited in the mesenteric glands. And we would consider those depositions of purulent matter in the viscera, which have been described by Velpeau, Andral, Carswell, and many others, as occurring without previous inflammation of the part, quite analogous to the state of the lymphatic ganglia, in cases of purulent absorption. The lung, liver, spleen, and thymus, may be to the veins what the ganglia are to the lymphatics. The viscera on the one hand, and the ganglia on the other, act in purifying the fluid which traverses them ; partly by a process of addition, and partly by one of separation. Fohman's conjecture that the lymphatic glands serve to aerate the lymph, is strengthened by the fact, that the sac-like appendages of the venous system in white tissue animals, diminish in number and size as we ascend in the scale ; in other words, as the lymphatic ganglia become more developed.

The analogies between lymphatics and veins may be further illustrated by the fact, that both contain fluids with properties not unlike ; fluids separable into a crassamentum and watery portion, and capable of receiving an important change from the action of air. Finally, the intimate connexion and anastomoses of the *minute lymphatics* with veins, has been demonstrated by Meckel, and described by him in his splendid work, dedicated to Soemmering.

We shall conclude this notice by the following important extracts, and in particular call the attention of our readers to the analogy drawn between the reproductive powers of parts in the lower animals, and the same phenomenon in the white tissues of the higher.

“ The nutrition of the red parts is accomplished by the constant circulation of red blood through them, that of the white parts, by the circulation of the serous portion of the blood ; and to effect these two different purposes, a portion of the serum is separated in the smaller vessels. When, however, the red blood and the white blood have circulated through the red and the white parts, there is no longer any necessity for their being kept altogether distinct from each other ; in the *mammalia* the *conglobate glands*, in the first instance, serve to promote the reunion of the red and white blood, and the larger lymphatics, which open into the subclavian veins, finally complete the reunion of these two portions of the blood. In fishes, Fohman has pointed out numberless communications between the venous and lymphatic system, almost at the roots of the latter.

“ The white structures of the higher animals, resemble the solids of white-blooded animals, not only in health, but disease. Thus the power of *reproduction* of parts destroyed by accident or disease, so remarkable in the lower orders of animals, is in the higher enjoyed only by white structures, such as cellular membrane, for proper muscular fibre when once destroyed is not reproduced, condensed cellular

membrane being employed to repair solutions of continuity, in this as well as all more highly organized tissues.

“ In white-blooded animals, we often see a new limb appear in the place of one destroyed by accident, and in man it is not unfrequent to observe a *new white organ* produced when the old has become useless, or been destroyed. Thus in unreduced dislocations, we have new bursæ mucosæ, capsular ligaments, synovial membranes, &c. produced so as to form almost all the appendages necessary either to the strength or motion of the new joint. The same happens in ununited fractures. Cartilage is thrown out to supply the place of bone removed by operation or disease, and under favourable circumstances, this cartilage itself becomes ossified, and, as happens in Necrosis, an entirely new bone is sometimes produced. In all such cases, the mould of the bone, or that part of it to which the new bone owes its form and bulk, is composed of a white structure, chiefly coagulated albumen: this is first formed, and afterwards the bony particles are deposited in it from red vessels.

“ This facility of reparation forms a very striking analogy between the white parts in man and other red-blooded animals, and the general structure of the solids in white-blooded animals. In point of vitality, the analogy is most striking. The white parts in man, when not inflamed, (then they for a time become red parts, and have a corresponding increase of vital energy,) enjoy but a low vitality. They are scarcely, if at all, sensible; do not possess irritability; and probably, also, the circulation of the white blood through them is much slower than that of the red blood through the red parts; at least the circulation of the *white venous blood in the lymphatics* appears much less rapid than that of the red venous blood in the veins.”—Page 19.

“ The view already taken of the intimate connexion in all the different classes of animals, between the development of the white parts and that of the lymphatic system, is easily explained on the supposition of the lymphatics being the veins of the white parts. In invertebrated animals which have no red blood, it would be more rational to call the vessels conveying the white blood back to the heart, lymphatics, than veins: for it is more consistent with analogy to suppose, that in the lower animals, the retained portion of the circulating system corresponds with the former, as in the superior animals the lymphatics are connected with parts, which, in their degree of vitality, most resemble the structures of the lower animals.

“ In the higher classes of animals, there are not only two circulating systems, one of red blood, and another of white blood, but also a two-fold system of nerves, the cerebral and the ganglionic; the latter of which, in invertebrated animals, seems to perform all the nervous functions necessary to their state of existence; while these animals are also remarkable for possessing only a simple vascular system.

“ We find, therefore, a correspondence between the vascular system of red blood, and the nervous system of the brain and spinal marrow. They are most perfect in animals most abounding in high-

ly aerated red blood, and decrease according to the descending scale proportioned to the decrease of red blood, until at last we arrive at the invertebrated animals, possessing no red blood, and no brain or spinal marrow."—Page 24.

We trust that Dr. Graves will still further elucidate the facts and opinions contained in this paper ; facts and opinions which, we scruple not to say, contain the germs of explanation of many points in the anatomy of formation, and in the higher parts of pathology ; particularly those referring to the scrofulous diathesis and other arrests of development.

W. S.

Principles of Geology. By CHARLES LYELL.

(Continued from Vol. III. page 417.)

RESUMING our observations on this important work, we shall endeavour to give our readers a still clearer perception of its value and excellence, by placing before them in the strongest light the principles it inculcates, and we are the more induced so to do because there is no habit, whether in moral or physical science, more common than that of looking at the objects before us, just as if they had never passed under the modifying influences of long-continued circumstances or forces. In moral science this habit is replete with evil, as it leads to erroneous reasonings founded on the supposition of originality in the condition of certain characters and propensities, which have in truth been forced into an unnatural development by artificial training ; and in physical science it has equally operated against the true understanding of the laws of nature, by diverting the observer from an inquiry into the various states through which each object, susceptible of change, must in the course of ages have passed. Geology has perhaps more than any other science suffered from this habit, for in the production of all its visible results, time has been so overpowering, so essential an element, that it was often found or thought impossible to connect the ultimate with the primitive condition of the object observed ; for instance, to see the materials of the sandstone and conglomerate rocks, now elevated into extensive mountain chains, hurrying along under the influence of tidal or other currents, and depositing as sand or shingle banks at the bottom of the ocean ; to trace in the long ranges of limestone rocks, full of organic remains, the vestiges of ancient lakes or estuaries ; to follow back the successive steps of increment, by which the present valley, wide and deep as it now is, may be proved to have commenced in a shallow

and minute ravine, the bed of a stream, the ceaseless action of which has produced so vast an excavation that its waters are lost in the very depth of its own works, or to see in the granitic mountain and the basaltic peak or ridge, melted matter either simply projected upwards, and cooling into domes and pinna-cles, or flowing in streams from the submarine crater, condensed and restrained by the pressure of a superincumbent ocean. Yet to read rightly the records of past ages, and to connect them with the history of the present, it is essentially necessary to free our minds from the prejudices of habit, and to adopt a course of inductive reasoning which will enable us to discover analogies, where we should at first sight discern only dissimilarity and discordance. The great object of the work before us is, to effect in geology this desirable end, and we may fairly say, that our author has to a great extent, if not entirely, attained his purpose. In our preceding remarks we entered with him on the question of the igneous origin of granite, for to the early geologists the highly crystalline arrangement of its component parts proved a serious stumbling block, since they deemed it only reconcileable with deposition from some solvent menstruum, being thereby obliged to call into being fluids different in their chemical properties from any now known to us. The experiments of Hall and Watt have, however, removed these difficulties, and the careful examination of granite and porphyritic dikes, has left no room for doubt as to the igneous origin of the great masses of rock from which they have emanated.

“ Granite, porphyry, and other rocks of the same family, often occur in large amorphous masses, from which small veins and dikes are sent off, which traverse the stratified rocks called primary, precisely in the manner in which lava is seen to penetrate the secondary strata. We find also one set of granite veins intersecting another, and granitiform porphyries intruding themselves into granite, in a manner analogous to that of the volcanic dikes of *Ætna* and *Vesuvius*, where they cut and shift each other, or pass through alternating beds of lava and tuff.”—Page 353.

And this statement our author supports by several instructive and illustrative drawings, principally taken from the works of M'Culloch.

The more, indeed, we contemplate these phenomena, and the wider the range of characters we take into consideration, the more firmly shall we believe in the identity (as to mode of formation) of plutonic and volcanic rocks. Prismatic structure, veins and dikes, disturbance and distortion of the intersected strata, and sensible alteration of the substances in contact with them, are alike common in both classes of rocks. The remarkable dikes of the *Val del Bove*, projecting like huge ver-

tical walls from the flanks of *Ætna*, are good examples of the first step of the analogical series beginning in the products of still active volcanoes, and after passing through the more ancient basalts and greenstones terminating in granite.

“Some of these are composed of trachyte, others of compact blue basalt with olivine. They vary in breadth from two to twenty feet and upwards, and usually project from the face of the cliffs. They consist of harder materials than the strata which they traverse, and, therefore, waste away less rapidly under the influence of that repeated congelation and thawing, to which the rocks in this zone of *Ætna* are exposed. The dikes are for the most part vertical, but sometimes they run in a tortuous course through the tuffs and breccias.”—Page 90.

Approximating, therefore, in one case to ordinary trap dikes, and in the other to veins; or if we turn to the other end of the series, we find, that,

“In the plutonic as in the volcanic rocks there is every gradation from a tortuous vein, to the most regular form of a dike, such as those described as intersecting the tuffs and lavas of *Vesuvius* and *Ætna*. In the dikes of granite which may be seen, among other places, on the southern flank of *Mount Battoch*, one of the *Grampians*, the opposite walls sometimes preserve an exact parallelism for a considerable distance.”—Page 357.

So that whatever part of the series we examine in this particular, the analogy is complete. In like manner examples of change of structure, produced by the intrusion of igneous rocks amongst, and their contact with sandstones and limestones, may be taken either from volcanic, from trap, or from plutonic rocks, as in the *Cornish granites*, where “changes are sometimes caused in the intersecting strata, very analogous to those which the contact of a fused mass might be supposed to produce;”—page 355; in *Glen Tilt*, where the limestone is so affected by its approximation to the granite, that “the crystalline texture disappears, and it assumes an appearance exactly resembling that of hornstone; the associated argillaceous schist also often passing into horn-blende slate;” and in the department of the *Hautes Alpes*, in France, near *Vizille*, where *M. Elie de Beaumont* traced a black argillaceous limestone charged with belemnites, to within a few yards of a mass of granite.

“Here the limestone begins to put on a granular texture, but is extremely fine grained. When nearer the junction it becomes grey, and has a saccharoid structure; and in another locality near *Champoleon*, a granite, composed of quartz, black mica, and rose-coloured felspar, is observed partly to overlies the secondary rocks, producing an alteration which extends for about thirty feet down-

wards, diminishing in the inferior beds which lie farthest from the granite. In the altered mass the argillaceous beds are hardened, the limestone is saccharoid, the grits quartzose, and in the midst of them is a thin layer of an imperfect granite. It is also an important circumstance, that near the point of contact both the granite and the secondary rocks became metalliferous, and contain nests and small veins of blende, galena, iron, and copper pyrites."—Page 371.

When then we see these analogies in the more striking of their phenomena, and find also a gradation between trap rocks and granites, effected by the addition or subtraction of some of the usual component minerals, we are prepared with our author to admit, that

"It would be easy to multiply examples to prove that the granitic and trap rocks pass into each other, and are merely different forms which the same elements have assumed, according to the different circumstances under which they have consolidated from a state of fusion."—Page 362.

But arrived at this important conclusion, we have yet to inquire, whether the varying forms of igneous rocks have resulted from a variation of the forces, which have impelled them successively to the surface, giving rise at remote periods to granites, then to traps, and last of all to recent lavas; and the inquiry leads us to one of the most interesting of modern geological investigations, the age of granites. It is here, perhaps, scarcely necessary to remind the reader, that the term primitive applied to this rock, and to gneiss, mica slate, &c., had its origin in the theory of their prior formation, and consequently, that they were supposed to underlie all other rocks. But in this as in most of the early applications of philosophical reasonings to practical science, generalization commenced before a solid foundation for it had been laid in the accumulation of facts, and as a consequence, more extended observations have overthrown many such hasty deductions, or at least modified greatly their import. That granite as a mass, or indeed any other igneous rock, should be found under those strata which have been formed out of the shattered and rolled fragments of pre-existing rocks, or by the accumulated relics of successive races of organized beings, is a necessary consequence of its mode of formation, for within the crust of the earth could alone be found an adequate source of heat, to produce the required fusion. But the time of appearance on the surface is distinct from that of original position or formation, and here it is that modern discovery has thrown a new light on the subject, and proved by the study of granite and porphyritic veins, both in

their passage through and the disturbance they effect on stratified deposits; and also, by a close examination of the changes from conformability to unconformability in the strata of mountain ranges, that the epochs of eruption of granite have been as numerous, and as widely different in point of age, as those of trap or even of volcanic lavas. The instance we have already adduced of a granite near Champoleon, which in part overlies oolitic strata, is sufficient to illustrate this important truth, but it will be well also to quote the words of Brongniart, *Tableau des Terrains*, page 329, as propounding it in the most distinct manner:—"These results of observations, at once varied and intricate, although singly received with caution, acquire, when combined together, such force, as to induce an admission, that the plutonic granite (particularly that with a porphyritic structure) has appeared at several distinct epochs on the surface of the earth, covering each time some portion of the strata which then formed the exterior surface of the globe." Brongniart mentions three such epochs, viz. after the deposition of the transition strata, after that of the coal formation, and between the new red sandstone and the inferior oolite. We shall add also, from a report of Humbolt's recent *Travels in Asiatic Russia*, an example which, from its magnitude, deserves especial attention:—*Annales des Sciences Naturelles*, vol. xxiv. p. 228. "In returning from Khoni-Mailakhon to Ust Kamenogorsk, the travellers traced for more than 5000 metres along the secluded banks of Boukhtarma, granite divided into almost horizontal beds, and spread over a slate, the strata of which are in part inclined at an angle of 85° in part horizontal."—No longer then is it mere conjecture, but a fact established on the clearest evidence, that "some granites and granitic schists are of origin posterior to many secondary rocks," p. 374. In other words, "some primary formations can already be shewn, to be newer than many secondary groups, a manifest (as our author further observes) contradiction in terms;" to remove which he proposes the word "hypogene," or nether-formed, as a substitute for primitive or primary. Having thus satisfied ourselves that granites, like trap rocks, have been brought to the surface of the earth at several different epochs, and contemplating the great extent of that surface they occupy, and the connexion which the eruption of such large masses must have had with its configuration, we are led from the rock to the mountain, from a consideration of the age of the mineral mass to that of the mountain chain. Not indeed that we are to expect to find each instance of elevation accompanied by a visible development of igneous rocks, any more

than we find each shock of an earthquake accompanied by volcanic eruption. That such rocks should be poured out requires a rupture of the earth's crust, and it may so happen, that as in the phenomena of recent earthquakes and volcanoes, the shock and elevation of the surface may have taken place at a distance from the spot, where through less cohesive materials, the struggling and semi-fluid mass was enabled to force a passage. In this way we may understand, how in some cases the elevation has affected a great extent of surface, giving rise to high table lands, in others being limited in breadth, has produced a greater amount of rise in mountain chains. In some has rent, torn, and twisted the strata, without the appearance of melted matter, whilst by veins and dikes, its proximity to the site of disturbance has, in others, been rendered apparent. To Mr. Elie de Beaumont is due the merit of attempting to collect together the facts bearing on this truly interesting branch of geological science, and to "reduce them to one systematic whole." In his memoir read before the French Academy, and subsequently published in the *Annales des Sciences Naturelles*, he details the results of his inquiries, and establishes, by a consideration of the particular part of the series of strata in each chain, where conformability between the successive layers has ceased, ten distinct revolutions or epochs of elevation. Subsequently, as stated by M. De la Beche, he has distinguished twelve such epochs, it being a main principle of his theory, that the operation of the force producing each elevation, has extended to all those mountain chains, in which the breach of continuity in the strata appears, at the same points of the geological scale, and further, that a parallelism exists in the direction of the several lines of every distinct and contemporaneous elevation. As this theory is based on the hypothesis of revolutions so sudden and violent, as to give rise at a single effort to great mountain chains, the moving force acting with fearful intensity after long intervals of repose, it differs essentially from that of our author, in which is advocated a frequent repetition of moderate efforts, corresponding with observed facts, and connected with "the intermittent action of subterranean volcanic heat, as a known cause capable of giving rise to the elevation and subsidence of the earth's crust, without interruption to the general repose of its habitable surface," p. 339. M. Elie de Beaumont does indeed, like our author, suppose that the causes which once acted may act again, for, *Annales des Sciences Nat.* tom. xix. p. 233, referring to the results of his memoir, he observes, "it is difficult to foresee such a modification as would lead us to suppose, that the mineral crust of the terrestrial

globe has lost the property of being wrinkled up in various directions, and that the period of tranquillity in which we now live will not again be disturbed by the appearance of a new system of mountains, resulting from a new dislocation of the surface of the earth, the foundations of which are yet, as earthquakes sufficiently warn us, capable of being shaken." But in substituting "paroxysmal violence for the reiterated recurrence of minor convulsions, and in ascribing the instantaneous rise of mountain chains to the secular refrigeration of the internal nucleus of the earth, he widely departs from our author both in seeking new causes, and in attributing to others a kind and a degree of action, not in accordance with the visible effects of these which are yet in operation. In the 24th chapter, our author discusses this question of difference at length, adducing the arguments of Bouè and others against the correctness of some of M. Beaumont's facts, and the serious objection taken to the doctrine of parallelism as an evidence of contemporaneity, from M. Beaumont's important admission, that "the elevating forces, whose actions must be referred to different epochs, have sometimes acted in Europe in parallel lines," p. 346; asking, "on what principle then is the age of an Indian or trans-Atlantic chain referred to one of these European lines rather than another?" p. 347. Professor Sedgewick has, indeed, declared his adhesion to the opinions of M. Beaumont, although he has remarked, "that in consequence of the spheroidal figure of the earth, different mountain chains, running north and south; cannot be strictly said to be parallel, since they would, if prolonged, cross each other at the poles." page 347. And M. Conybeare "points out many lines of distinct ages in England, which are exactly parallel, and others which, according to the rules laid down by M. de Beaumont, ought to agree in age with certain continental chains, and yet do not, having an entirely different direction," p. 348. He imagines also, "that the general strike of the secondary strata of our island, from N. E. to S. W. has been the result not of any violent or single convulsion, but on the contrary, of a gradual, gentle, and protracted upheaving, continued without interruption during the whole period of the formation of all these strata," and in like manner that in mountain chains, their rise is attributed with greater reason to a series of shocks occurring at intervals, "through a long period of ages," than to one blow, since "such an hypothesis is most conformable to the only analogy presented by actual causes, "the operation of volcanic forces." We shall not dwell further on the question of parallelism in contemporaneous mountain chains, because to

us it appears evident, that any force of the volcanic kind, sufficient to thrust up at one effort the mountain ranges of different and distant countries, must (to obtain the power of producing such an effect) have been so deeply seated in the interior of the globe, that its action would necessarily have proceeded from a centre, and been manifested on the surface in lines corresponding with, or at least approximating to great circles of the sphere; and adopting the theory of refrigeration of the external crust, accompanied by its contraction and a consequent pressure on the internal melted mass, when the crust had been, by the resistance of that nucleus to its further contraction, fractured, (just as a glass might be by the unequal contraction of its external and internal coats,) and the melted matter forced by pressure through the opening, a theory which we are far from thinking mysterious, we should still expect to find the cracks, if regular, corresponding to circles of the sphere; though doubtless, they would in nature be irregular from the various modifications dependent on unequal cooling in different parts. But even should we reject the doctrine of parallelism, the question of instantaneous rise would not be affected by that determination, and as yet we think the data afforded by actual observations insufficient to decide it. Our author's arguments shake indeed M. de Beaumont's conclusions, but to overthrow them entirely, it is necessary to examine minutely the appearances of the elevated and disturbed strata, and to connect them with an efficient cause, in other words to coordinate cause and effect. And we enforce this, because the occasional manifestation of extraordinary energies arising out of accumulated forces appearing as consistent with the analogies of nature as an extraordinary tide, a hurricane, or an unusually tremendous earthquake may be, we should expect to discover, in the disturbance of strata, evidences both of moderate and of excessive violence. This class of inquiry has now been entered upon, and in the work of M. Thurmann, on the "*Soulèvements Jurassiques du Porrentruy*," we find the several phenomena of disturbance classified, according to the several kinds or forms of elevation, and the various accidents which effected them. Of the ultimate bearing of this work, (a small portion of it only having as yet appeared,) on the question at issue, it is difficult to predicate: in the part before us, the author speaks indeed of the gigantic elevations of the Jura, but we imagine with reference to the aggregate result, since he observes: "it is only after having studied the whole of the vast system of the Jura, that we shall be able to form a correct idea of each of the partial elevations which have contributed to its production,"

p. 69. It seems indeed impossible to contemplate the general surface of the globe, raised as it has all been from the bosom of the ocean, without perceiving, that the mountain chain must at one time have participated in the elevation of the plain, at another been detached from and raised above it by the concentration of the uplifting force within a narrow compass; and the more we succeed in discerning distinct lines of direction, however partial, in the phenomena of disturbance, the more reason we shall have to suppose, that the great resulting elevation has proceeded from a combination of minor and successive elevations. In short if we hesitate to admit the instantaneous elevation of a continent, why should we advocate that of one, nay, of many mountain chains? Enough, therefore, we think, has now been said, to satisfy our readers that in this portion of our subject, our author has demonstrated, to a certain extent, a similarity and consistency in the operations of nature (both in kind and degree) at all times, for we have seen that granites and other igneous rocks have been poured out at various epochs, assuming this or that character, according to attending circumstances; and can therefore readily admit that at this moment, under the pressure of a superincumbent ocean, they may still be formed and ejected; and in like manner when we have traced with him the numerous stages through which our continents and mountains have passed towards their present elevation, we cannot doubt, that other rocky masses are on their way from the deep recesses of the sea, to form hereafter new continents and new mountains, when those now in being have been gradually worn away, or sapped at their base by the long-continued action of subterranean forces, have again sunk to the depths whence they came. Turning from igneous rocks, and the phenomena which have attended their production and elevation at various epochs, to the surface of the earth, we have now to consider, whether a similar degree of analogy can be discovered between the ancient primary strata, and more recent sedimentary and stratified deposits; and further, how far such formations generally can be shewn to accord with the productions of existing and still acting causes. In some of our preceding remarks, we have already placed the sandstones and conglomerates in juxta-position with sand and shingle banks, now formed or forming along our shores, and to support the analogy, we may appeal to examples amongst the secondary strata, on one hand, of masses of sand scarcely consolidated into stone, and on the other to the shores of the Mediterranean, or even of the channel, (as at Kingstown,) of sand and shingle, either actually consolidated or in course

of being so. But can we in like manner find amongst recent productions, a type for such rocks as the primary schists. gneiss, mica-slate, clay slate, or even for those extended fields of limestone which abound in this island? At first sight it would appear impossible to do so, for where should we seek a modern formation, any part of which could be compared with gneiss,—a compound rock at once crystalline and stratified, resembling granite so closely in structure, that in hand specimens it is often undistinguishable from it, and at the same time approximating in stratified arrangement to the sedimentary deposits. But in this as in every geological investigation, we must keep in view the fact, that the surface of the earth, as we now see it, exhibits not one class only of phenomena, but an assemblage of many, and draw from it a caution not to confound together those which in their nature are distinct: to compare for instance, the modern sedimentary deposits, consolidating at a small depth below the surface of the sea or of lakes, by the infiltration and absorption of mineral matter, with rocks which have been formed at the lowest depths of the ocean, under the combined influence of enormous pressure and of intense heat. To institute then a comparison with gneiss and other stratified primary rocks, we should have to restore the circumstances under which they were produced, to refer to strata forming under a similar pressure and in proximity to a similar source of heat, and to operations veiled from us by the ocean which is an element essential to their development. It is on this principle that our author details the marked analogies between primary and other formations, which are displayed in their mode of stratification, particularly in the alternation in each of beds, varying greatly in composition, colour, and thickness.

“ In the secondary and tertiary strata of pure siliceous sand, alternating with micaceous sand and with layers of clay, as in the primary beds of pure quartz, with mica schist and clay slate. In the secondary and tertiary series, limestone alternating again and again with micaceous or argillaceous sand, just as in the primary, gneiss and mica schist alternate with pure and impure granular limestones:”—page 366.

And adopts the theory of Hutton, as that which reconciles best these features of agreement, with the strong marks of distinction found in the highly crystalline and granitic structure of primary rocks.

“ If, says he, reasoning from the principle that like effects have like causes we attribute the stratification of gneiss, mica-schist, and other associated rocks, to sedimentary deposition from a fluid, we en-

counter this difficulty, that there is often a transition from gneiss, one of the stratified series, into granite, which, as we have shewn, is of igneous origin. Gneiss is composed of the same ingredients as granite, and the texture is equally crystalline. It sometimes occurs in thick beds, and in these the rock is often quite undistinguishable in hand specimens from granite; yet the lines of stratification are still evident. These lines imply deposition from water, while the passage into granite would lead us to infer an igneous origin. In what manner can we reconcile these apparently conflicting views? The Huttonian hypothesis offers, we think, the only satisfactory solution of this problem. According to that theory, the materials of gneiss were originally deposited from water in the usual form of aqueous strata, but these strata were subsequently altered by their proximity to granite, and to other plutonic masses in a state of fusion, until they assumed a granitiform texture."—Page 367.

Our author quotes several examples of alterations in texture, by the contact of trap and volcanic rocks with sedimentary deposits, as illustrative of this change, and having referred also to the "direct experiments on the fusion of rocks in the laboratory," proposes the term "metamorphic" for stratified primary rocks, as a designation involving no chronological import, and pointing to the change which is presumed to have taken place. This theory is indeed supported by many remarkable facts; for instance,—

"In parts of Germany, also, there are schists which, from their chemical condition, are identical with hypogene schists (*id est* primary schists), yet are interstratified with greywacke, a rock probably modified by heat, but which contains casts of shells, and often displays unequivocal marks of being an aggregate of fragments of pre-existing rocks.—Page 376.

And if true, there may have been many formations of such rocks going on at various and distant periods. But reasonable as it in many respects is, some difficulties do yet beset it. For example, when our author says,

"A considerable difficulty and misapprehension, in regard to the antiquity of the metamorphic rocks, may arise from the circumstance of their having been deposited at one period, and having assumed their crystalline texture at another. Thus, for example, if an eocene granite should invade the lias, and superinduce a hypogene structure, to what period shall we refer the altered strata? Shall we say, that they are metamorphic rocks of the eocene or liassic eras? They assumed their stratified form when the animals and plants of the lias flourished; they became metamorphic during the eocene period;"—Page 378.

we must at once see that the circumstances detailed are rather those which attend the intrusion and passage of basaltic dikes, than those which have been premised as indispensable to the production of gneiss, mica slate, and other stratified primary rocks. Neither the pressure attendant on great depth, nor the long-continued heat arising from contact with a great mass of heated matter being here provided, since in the case of the lias, the peculiar relics of organic beings found in its strata, and the mode of their arrangement, shew that they were in great measure formed in estuaries, and in shallow waters. But the fault here is rather in the argument than in the theory, and we shall therefore state briefly the difficulty which really embarrasses it. To account for the elevation and protrusion of large quantities of melted stoney matter, by the action of any of those forces usually supposed to operate in volcanic eruptions, such as gases or steam suddenly generated, and excited to powerful expansion by intense heat, we require some external covering, which by vaulting over the heated nucleus, may hold in the elastic vapours until their explosive energy has been sufficiently augmented to enable them to burst at the weakest point the retaining shell, and to force through the opening the nearest portion of the melted matter in which their pressure has been exerted. In all recent eruptions, whether of basalt or of lava, this retaining force is exerted by the great mass of the secondary and tertiary strata, but if we attribute a secondary origin also to gneiss, with what are we to suppose that granite, appearing as it does at almost every point of the earth's surface, was coated, and if uncovered, in what way was its elevation effected? Surely then we have reason to think that there may have been an epoch when gneiss rocks alone were in being, and to examine, therefore, with attention the theory of Cordier, and of De Beaumont, which consistently begins at that point. According to it we should suppose the external crust of the heated globe cooling and consolidating more rapidly than the internal mass. It would then begin to contract and press on the heated or melted matter within, until fractured by its resistance to further pressure; and in this way we should have a stratified rock, such as gneiss, formed before any great expulsion of the unstratified could be effected. Nor does the division into thin strata of this rock militate against our view of the subject, since we can readily imagine the formation in the first instance of a very thin layer, which would yield immediately to pressure, and suffer by the fracture only a moderate degree of disturbance; then of another and another within it, each layer by accumulation adding to the retentive power of the crust, and, consequently, to the magnitude of the effort necessary to burst

it asunder. But we shall pursue this inquiry no further, since it is certain that on whatever principle we base our theories, the fact that in secondary rocks formed, as the abundant relics of organic beings testify, at moderate depths, we do not find exact types of the primary crystalline strata, which we believe to have been formed at great depths, does not in any way remove the latter from the strict analogies of nature, as displayed in its operations at all periods and in all places.

Quitting the consideration of primary rocks, we shall now briefly advert to another great class, the several members of which have in common this striking feature of resemblance—that they contain the relics of animal life, either in forms now no longer existing, or in those still familiar to us; we allude to the successive formations of stratified limestone. Before, indeed, the study of organic fossils had attained that degree of perfection which now characterizes it, the searcher for analogies would have met in these rocks a serious stumbling block, but modern observation and discovery have afforded a clue to every difficulty. Our author, in his *Memoir on the recent Fresh Water Lake Formations of Scotland*, read before the Geological Society of London, and published in its Transactions, detailed the process by which limestones were, as heretofore, produced by the gradual accumulation of the exuviae of testaceous mollusca, and in the preceding volumes of this work, he brings forward many examples of more extensive formations of the same kind: but were the superficial extent of such deposits even more considerable than it generally is, we should have (excepting occasionally in coral reefs) few means of deciding their thickness. To future generations must be left the determination, for it is more than probable that they will see these now hidden strata elevated in their turn to light; and in our inquiry we must begin with the strata next in order, (or those most recently uplifted), and endeavour, if we can, to connect them with the present order of things. The tertiary strata afford us the desired link of connexion, as in the several parts of that truly interesting series of deposits, we find a gradual approximation in the characters of the imbedded organic fossils, to those of still living testacea, until in the more recent strata they become, in the greater proportion identical. In like manner also in their superficial extent they may be considered intermediate between the widely spread masses of the secondary strata, and the small patches of recent lacustrine deposits.

“The fossil remains,” says our author, “which characterize each of the successive periods alluded to, approximate more nearly to the assemblage of species now existing, in proportion as their origin is less

remote from our own era, or in other words, the recent species are always more numerous, and the extinct more rare in proportion to the low antiquity of the formation."—Page 21.

And when asking whether no passage can be traced from the most recent fossil condition of the once animate creation, to that which now prevails, he replies,

"It will appear in the sequel, that such monuments are not wanting, and that there are marine strata entering into the composition of extensive districts, and of hills of no trifling height, which contain the exuviae of testacea and zoophytes, hardly distinguishable as a group from those now peopling the neighbouring seas. Thus the line of demarcation between the actual period and that immediately antecedent, is quite evanescent, and the newest members of the tertiary series will be often found to blend with the formations of the historical era."—p. 22.

The limited range, also, of tertiary deposits, when tested by the organic remains they respectively contain, is at once a consequence and a proof of the gradual change of circumstances effected by their elevation.

"It is evident that large parts of Europe were simultaneously submerged beneath the sea, when different portions of the secondary series were formed; because we find homogeneous mineral masses, including the remains of marine animals, referrible to the secondary period, extending over great areas: whereas, the detached and isolated position of tertiary groups, in basins or depressions, bounded by secondary and primary rocks, favours the hypothesis of a sea interrupted by extensive tracts of dry land."—p. 23.

Could we indeed watch the change from a wide spread ocean, offering no barrier (except what might attend climate or temperature) to the extension of marine animals; to that of numerous smaller seas, estuaries, and lakes, cut off from each other by large tracts of intervening land, we should have no difficulty in seeing why

"The organic remains would be also more varied; for in one locality, fresh water species would be imbedded, as in deposits now forming in the lakes of Switzerland, and the north of Italy: in another marine species, as in the Aral and Caspian: in a third region, gulfs of brackish water would be converted into land, like those of Bothnia and Finland in the Baltic: in a fourth, there might be great fluviatile and marine formations along the borders of a chain of inland seas, like the Deltas, now growing at the mouths of the Don, Danube, Nile, Po, and Rhone: along the shores of the Azof, Euxine, and Mediterranean."—p. 25.

And

“ If such be, in a great measure, the distinction between the circumstances under which the secondary and tertiary series originated, it is quite natural that particular tertiary groups should occupy areas of comparatively small extent.”

We might, even from these considerations, derive a wholesome caution in the application of organic remains in such strata as the lias, as a test of age, since, with our author, we may fairly believe that they do prove the existence of similar circumstances, but not that those circumstances existed at the same moment of time. It would be impossible in our limited space to follow our author through all the details of this beautiful, and we may say, new-born portion of geological science ; which, since it was brought fairly into light by Cuvier and Brongniart in their description of the basin of Paris, has grown, with every step of its development, more interesting and more important. He distinguishes in the tertiary strata four distinct periods, for which he constructs names, founded on the approximation in number of species of testacea identical with those now living ; viz., *pliocene* from *πλειων*, *major*, *καινος*, *recens* ; *miocene*, from *μειων*, *minor*, and *καινος*, *recens* ; *eocene*, from *ηως*, *aurora*, and *καινος*, *recens*, because the extremely small proportion of living species contained in these strata, indicates what may be considered the first commencement, a dawn of the existing state of the animate creation ; the four divisions being, therefore, the newer pliocene, the older pliocene, the miocene, and the eocene ; and when we reflect that all these divisions and their strata, are subsequent to the chalk, we must feel no small degree of admiration at the rapid progress, within our own time, geological science has made.

The numerical proportion of shells in these stages of the tertiary period identical with living species, is as follows : 1st, thirteen still living, and common to all the periods ; 2nd, of the newer pliocene species nine-tenths are recent, or still living ; 3rd, of the older pliocene more than half, of the miocene more than a third, and of the eocene about a thirtieth part. The evidence therefore afforded, by the fossil remains of the tertiary strata, of a gradual approximation to the present order of things, is striking, and when we further consider their occasional great thickness, for instance, in the sub-Apennine deposits, or even in those of the Isle of Wight, where, according to Mr. Webster, as quoted by Mr. Bakewell, “ the whole thickness of the beds at Alum Bay, which are nearly vertical, is not less than three thousand feet, comprising *fourteen hundred and eighty-one feet of strata above the chalk*,” we shall have no hesitation in

admitting, that the analogies of nature have suffered no sudden and absolute break, but are traceable by similar operations through the whole chain of geological evidences. If, too, we have referred to the operations of elevating forces for an explanation of some of the changes in the characters of sedimentary deposits, so also, we may see in the connection of those elevations with strata so recent, a proof that the same causes have continued to act almost, nay up to our own times ; and taking into consideration the effect of modern earthquakes, we may add do still act. That, however, the very exercise of any particular force may in time produce a modification of its action, cannot in sound philosophy be denied, and we, therefore, are not surprised to observe that our author defends himself from the charge of having

“ Endeavoured to establish the proposition, that the existing causes of change have operated with absolute uniformity from all eternity.”—Page 383.

We shall now, though with regret, conclude our observations, hoping that we have sufficiently elucidated the principles of this great work, to satisfy our readers, that in its pages the philosopher may find matter for the sublimest speculations, whilst the practical geologist will see his science assuming a more perfect and pleasing form, and, imbued by the spirit it inculcates, carry with him to the field a power of generalization, which will marshal into order the most strange and discordant appearances of nature.

JOSEPH E. PORTLOOK.

SCIENTIFIC INTELLIGENCE.

CHEMICAL AND PHYSICAL SCIENCE.

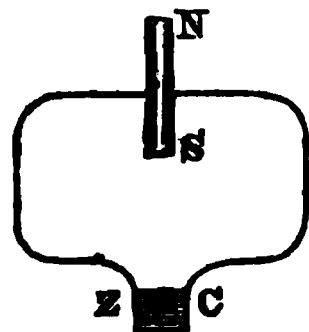
On the Development of Heat in the Flowers of the Caladium Pinnatifidum, by Doctor E. H. Schultz of Berlin.—Dr. Schultz gives the following account of experiments made by him to ascertain the elevation of temperature in the flowers of the *Caladium pinnatifidum*. “On the 1st of May this year, at mid-day, I had one of the flowers, which was beginning to burst, cut off from the plant, and found before and immediately after the separation, that the temperature of the flower was completely the same with the temperature of the air in the hot-house. I took the flower home with me, and placed it with the cut end of the stalk in a glass of water, in order to observe the phenomena during the period of its blooming. The temperature of the room was 13° R. ($61^{\circ}.2$ Fahr.), and the flower had likewise the same temperature until about five o'clock in the afternoon. About six o'clock, the flower, which had been previously without any smell, gave out a very powerful odour that reminded me of trying the temperature. This had risen 2° ; for that of the flower was 15° R. ($65^{\circ}.1$ Fahr.) At seven o'clock the temperature had risen to 17° ($70^{\circ}.2$ Fahr.) At eight o'clock to 19° ($74^{\circ}.7$ Fahr.); half-past eight, $19\frac{1}{4}^{\circ}$ (76° Fahr.). At nine o'clock $20\frac{1}{4}$ (78° Fahr.) At ten o'clock $21\frac{1}{4}^{\circ}$ (81° Fahr.); and this appeared to be the greatest height, since there seemed to be no farther increase up to eleven o'clock. During this elevation of temperature, the disengagement of the odour likewise increased; this became so powerful that the whole room was impregnated with an ammoniacal vapour. In the morning the temperature of the flower had again fallen to the temperature of the air. During the following evening no further elevation of temperature was manifested in this same flower. But, on the contrary, with other flowers which bloomed at a later period upon the same plant, there was the same gradual elevation of temperature and disengagement of odour, which afterwards, during the evening and night, impregnated almost the whole of the air in the hot-house. When this observation is compared with that of Lamarck and Senebier upon the *Arum Italicum*, and of Huber upon the *Arum cordifolium*, there appears a most striking difference in the period of the disengagement of the caloric; likewise in the quantity of caloric, which in the case of the *Arum cordifolium* rose from 19° to 44° , therefore 25° . Senebier found in

Geneva the highest temperature in the *Arum Italicum* to be developed at a quarter before seven in the evening. On the contrary, according to Bory de St. Vincent, the greatest elevation of temperature of the *Arum cordifolium* in Madagascar shows itself in the morning after sunrise about seven o'clock; and from eight o'clock the temperature gradually declines until the flower decays; so that in the evening the temperature of the flower differs but little from that of the atmosphere."

The *Caladium pinnatifidum*, which in Berlin shows the highest temperature of its flower about ten o'clock in the evening, grows wild in the shady forests of Caraccas. Hence it is possible that the periods of its disengagement of caloric, as well as the periods of its blooming, generally regulate themselves according to the peculiarity of the vegetation of the plant, as well as according to the climate of its native country.—*Edinburgh New Philosophical Journal*, Jan. 1834.

On the Reduction of Mr. Faraday's Discoveries in Magneto-Electric Induction to a general Law, by the Rev. William Ritchie, LL.D. F.R.S.—The law is founded on the universal principle that action and reaction are equal. Thus, if voltaic electricity induce magnetism under certain arrangements, magnetism will, by similar arrangements, react on a conductor, and induce voltaic electricity. Instead of stating the law of magneto-electric induction, and then illustrating it by experiment, it will be more instructive to arrive at it by a process of induction.

1. If a piece of soft iron, N S, be made to *approach* rapidly a voltaic conductor, and at right angles to its direction, the soft iron will be converted into a magnet having its poles developed at N S, according to the direction of the voltaic influence. If the battery, Z C, be removed, and the ends of the wire connected with the zinc and copper plates brought into metallic contact, and if the piece of soft iron be again *converted* into a magnet by means of a permanent horse-shoe magnet, the wire will have the *same* voltaic state induced on it as it had when connected with the battery.



If a temporary magnet be merely a piece of soft iron having the electricity essentially belonging to it arranged in a particular direction, it is obvious that the *motion* of the atoms of the electric fluid will take place in the *opposite* direction, when the iron is ceasing to be a magnet or returning to its natural state. Hence the direction of the voltaic influence induced on the conductor will, as Mr. Faraday has shown, take place in the opposite direction.

2. If two conductors, having electricity induced in them in the *same direction* by means of an elementary battery, be placed parallel to one another, they will be mutually *attracted*. Hence, if we remove one of the batteries, and connect the ends of the wires as in the first experiment, and then imitate *attraction*, by making the wires rapidly approach each other in a parallel position, the *same*

electric influence will be induced on the closed circuit as it had when connected with the battery. If the conductors, when connected with the batteries, have their electricities induced in *opposite* directions, they will mutually repel each other. Hence, if we remove one of the batteries, connect the wires as before, and *imitate repulsion*, by *removing* the wires rapidly from each other, the electric influence or current will be induced in the *same* direction as it was when the wires were connected with the battery.

3. If a voltaic conductor be made to revolve round the pole of a magnet, as in Mr. Faraday's first experiment on rotation, and if the battery be removed and the ends of the conductors brought into metallic contact, the *same electric state* will be induced on the closed circuit, by turning the wire rapidly round the pole of the magnet by mechanical force.

4. If we produce all the rotations described in works on electro-magnetism, and if we remove the battery, and bring the ends of the conductors into metallic contact, and then *continue* the rotation by *mechanical means*, the *same* electric state will be induced on the conductor which it had when connected with the battery.

In a paper of mine read before the Royal Society on the 21st of March, 1833, I first described the method of making a piece of soft iron or electro-magnet, either straight or in the form of a horse-shoe, revolve *rapidly* round its centre, either by the action of the earth, or of a horse-shoe magnet, by *changing* its poles *twice* in every revolution. Hence from the general law it follows, that if the soft iron be made to revolve by *mechanical force*, the *same* electric state will be induced on the conductor as it had when the ends of the wires were connected with the battery.

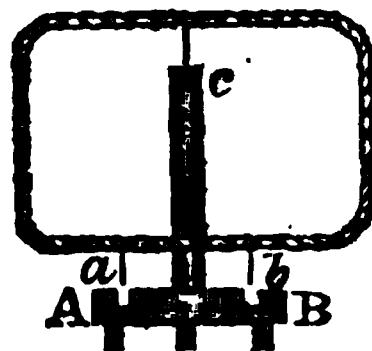
These facts were known, and the experiments publicly exhibited, months before the large revolving electro-magnet was exhibited in the Adelaide Rooms. The general law at which we have arrived may be thus expressed: "*If a wire, conducting voltaic electricity, produce, by its action on magnets or conductors, certain motions, as attractions, repulsions, or continued rotation; and if the battery be removed, the ends of the wires brought into metallic contact, and the same motions be produced by mechanical means, the conductor will have the same electric state induced on it, as it had when connected with the battery.*"—*Lond. and Edin. Philosophical Magazine*, Jan. 1834.

On the continued Rotation of a closed Voltaic Circuit, by another closed Circuit, by the Rev. William Ritchie, LL.D., F.R.S.
—M. Ampère has demonstrated that when a closed circuit (or a conductor of voltaic electricity returning into itself so as to form a complete circuit) is acted upon by another closed circuit, there is a determinate position in which *stable* equilibrium takes place. Hence the impossibility of producing continued rotation by the mutual action

of two closed voltaic circuits. Hence, also, the impossibility of producing continued rotation by the mutual action of two *permanent* magnets. But though continued rotation cannot be produced by the action of closed circuits, when the voltaic influence is exerted in a particular direction, I have succeeded in producing such rotation by changing the direction of the voltaic influence; a short account of which may not be unacceptable to the readers of the Philosophical Magazine.

The description of the method will be easiest understood by reference to the annexed figure.

Let AB represent the section of a circular piece of wood, having a groove measuring about an inch in its inner diameter, and half an inch broad, for the purpose of holding mercury. The groove is divided into two compartments by small slips of wood fixed diametrically opposite to one another. These compartments may be connected by means of wires with the plates of an elementary battery. A glass rod, having a small cup, *c*, at the top, is cemented into the centre of the sole of the apparatus. A fine copper wire, covered with silk, is formed into a rectangular coil or closed circuit, as in the figure, the ends of which, *a b*, dip into the mercury contained in the compartments. The lower horizontal branch of the rectangle has the wires separated so as to form an opening for the glass rod to pass through it, in order that the rectangle may hang perpendicularly. A similar rectangular closed conductor is supposed to be connected with the poles of another battery.



If the last conductor be placed above that represented in the figure, but not parallel to it as in the case of stable equilibrium, the moveable conductor will turn round till that position be gained. But the moveable conductor being put in motion will pass this position the moment the ends of the wires, *a b*, pass above the two divisions in the groove, so that the direction of the voltaic influence is changed, and the moveable conductor forced round another semicircle, when the direction of the influence is again changed; and so on, producing continued rotation. By using a magnet instead of the closed conductor a more powerful and rapid rotation may be produced. By placing the divisions in the magnetic meridian, the closed conductor may be made to revolve by the action of the earth.

This experiment, if I mistake not, will afford an interesting illustration of the mutual action of voltaic conductors, and of the striking analogy between a permanent magnet and a closed circuit conducting voltaic electricity.—*Ibid.*

Composition of Mummy Powder.—The following analysis of this powder has been submitted to the Royal Academy of Medicine by MM. Boudet, Boutron, Charlard, and Bonastre, who had been

charged by the Academy to examine the composition of an Egyptian mummy :

1. A resin entirely soluble in alcohol, and the characters of which could incline one to consider it as a species of turpentine which had been acted on by fire ;
2. A gum resin presenting all the properties of myrrh ;
3. Fragments of fruits, which by their appearance and analysis presented the greatest similarity to those of the nutmeg ;
4. Chloride of sodium and of lime ;
5. Lastly of a particular crystalline substance, belonging, however, to the mummy, and which analysis shewed us to be human margaric acid.

To the above analysis the following note is appended regarding the cinnamon of the ancients ;

We might here easily recognize *cinnamomum* if by *cinnamomum* we were to understand canella ; but Herodotus was perfectly well acquainted with canella ; he designated it, not by the name *κινναμωμον*, a word he never made use of, but by the term *καρφη*, which means small, dry, slender branches ; and the Arabs at this very day call canella *kerfê* or *qerfeh*. Thus for about 2250 years, canella was known in Egypt under the name of *καρφη*. What can it then be but cinnamon ?

The ancients supposed that the *κινναμωμον* originally belonged to those places where Bacchus was brought up, and that his name was given to it by the Phœnicians who received it from the Arabs. Now Bacchus' country is India, with which the Arabs communicated by means of the Red Sea. Desfontaine thinks that it is not possible to know the country whence cinnamon was derived, and that we cannot know to what tree the cinnamon belongs. Some authors think it to be the canella. Sprengel thinks it to be our *Laurus cinnamomum*. Stackhouse, on the contrary, conceives it to be *Laurus Cassia*. One of us, according to this analysis, which is in part confirmatory of the account given by the historians of antiquity, conceives that in the fragments of fruits resembling nutmeg we probably see the real cinnamon of the ancients."

Preparation of the Proto-tartrate of Mercury and Potash, by M. Carbonell.—M. Carbonell, who has paid particular attention to the treatment of syphilitic diseases, ascertained the antisiphilitic preparation of Pressavin (*rob antisiphilitique de Pressavin*) from which he derived the most signal advantage, owes its efficacy entirely to one of its ingredients called by Pressavin *eau vegeto-mercurielle*. This was merely a liquid arising from the combination of the oxide of mercury and cream of tartar by means of heat. M. Carbonell discovered, that the cream of tartar dissolved, with the help of heat, the oxide of mercury ; that thence there resulted two salts perfectly distinct, on the one side a simple salt, the tartrate of mercury, and on the other a double salt, which may be called the *proto-tartrate*

of mercury and potash. The following is what takes place during the action of tartrate of potash on the oxide of mercury. The excess of tartaric acid unites with the oxide forming tartrate of mercury, which is in part precipitated on cooling, whilst there is formed another salt, the proto-tartrate of mercury and potash, which remains in the liquid, and may be obtained by evaporation. The author's process is as follows:

1°. An intimate mixture is to be made by means of long continued trituration, and by adding a small stream of water, of one part of the protoxide or deutoxide of mercury, precipitated from the nitrate of that metal. This mixture is to be thrown by small proportions into an earthen pan, capable of containing at least sixteen pounds of boiling water for two of the mixture. This solution is to be passed, while hot, through a filter prepared on fine linen, which is to be placed in a frame of wood.

2°. This liquid being filtered, is to be made to crystallize, or else evaporated to dryness, taking care to separate all the tartrate of mercury which is precipitated, or separated under the form of pellicules or crystals; which is to be collected by means of a wooden or ivory spoon, as it presents itself. This operation is to be repeated as often as is necessary, especially at the end of the operation. The saline liquor concentrated, so as to give 30 degrees to the areometer of Baumé, and well filtered, contains no more or hardly any tartrate of mercury. Evaporation may then be continued to dryness, or made to crystallize, (if the liquor marks 52 degrees to the areometer above mentioned in forming pellicules) and the result is a proto-tartrate of mercury and potash, and very pure, crystallized or not.

3°. The evaporation to dryness may be made in a glass or varnished earthen-ware vessel, which is to be placed in a sand bath on a slow fire, or rather a water bath, so that the salt should not be altered or decomposed. The liquor is then to be left to dry, at the heat of the same evaporating bath, and excluding the light. It may be also obtained crystallized in tables, by exposing the saline liquor reduced to the least volume to a moderate heat, or what is preferable to a stove.

4°. The salt obtained ought to be enclosed in crystal flacons hermetically sealed, so that the salt may not attract humidity from the air, and these flacons ought to be covered with black paper, or enveloped in cloth of the same colour, for the salt might be easily altered, as happens in different preparations of mercury.

As to its medical use, the proto-tartrate of mercury and potash may be given dissolved in water, with the addition of *mel rosæ* or some syrup, provided it be not acid. It may also be given in the form of pills, made up with a little mucilage or extract, such as extract of Peruvian bark. It may be combined again with advantage, with the extracts of henbane, and wolfsbane, for complicated venereal pains, rheumatism, and again with the magistery of sulphur, when there is a complication of the herpetic virus.—*Journal de Pharmacie*, Nov. 1833.

NATURAL HISTORY.

Peculiarity in the Respiration of the Crocodile, by Captain Portlock, R.E., F.G.S., M.R.I.A.—In investigating the natural processes by which animal life is sustained, we must be cautious not to attach more importance to the organ, as an organ, than to the function which it is destined to perform, as in doing so we may check our progress in the discovery of other modes of effecting the same object. In respiration, for example, the function to be performed is the separation of oxygen from the air, either of the atmosphere, or contained in water, and of carbon from the blood; and though the ordinary instruments for effecting this exchange may be lungs, gills, or tracheæ, it by no means follows, that there are no other methods of spreading out the minuter blood-vessels, and facilitating the mutual action on each other of the blood within and the air without them. The numerous air-lets of birds may in part aid the function of respiration, and the tracheæ of insects may be considered as lungs, spread generally over the body, and operating directly on its fluids. We make these remarks, in order to introduce the passages in the “*Cours de l'Histoire Naturelle des Mammifères*,” by M. Geoffroy St. Hilaire, which describe a peculiar modification of the respiratory organs in the crocodile. After pointing out the unreasonableness of supposing, that those parts of the animal structure, the use of which we have not as yet discerned, are indeed without a use, and that those peculiarities in habits or manners of animals, which at times seem inexplicable, are without a cause in the animal structure, because we have hitherto failed to find it, he observes, page 9:—

“In this position, as regarded the crocodile, I found myself, for the observations of the ancients, and also of modern travellers, had represented him, when on land, as an animal so timid, that it was with wary caution he stole upon his prey, casting himself, on the least alarm, into the water; but when immersed in the water, as one so bold, vehement, and fearless, that he became the terror of all other animals. Reflecting on these facts, and considering that no extraordinary development of vital energy is possible, unless excited and supported by an equally powerful respiration, I was induced to inquire, how an animal breathing by lungs, fitted as they are for an aerial medium, should acquire this augmented respiration in an aqueous one. At length I discovered that there was a cause for the apparent anomaly, and that cause a most curious modification of organization, through which the crocodile enjoys not only the advantages of its ordinary means of respiration, as an animal breathing air, but of those also which are possessed by a large portion of aquatic animals. They breathe, when immersed in water, like the holothuria; their abdomen being converted, as it were, into a vast aquatic trachea.” And again, page 28, “Such indeed is the case of the

crocodile ; he possesses a lung more perfect than that of any reptile, and yet is little excited by its use, for on land, where the resources of his pulmonary organization are called into action, he is timid, feeling them insufficient to answer the demands of his instinctive ferocity. But let him enter the water and he is a changed creature : in that medium his vigour is extreme, his swimming rapid, and by the exhibition of all the ardour and force of a lion, he displays the energy which attends powerful respiration. Nor can it be in its pulmonary organization that the crocodile finds support for this excess of activity, since that organization has been found on land insufficient to such a purpose, whilst its use in water is necessarily suspended. Auxiliary organs enter into function : the crocodile breathes in water in the manner, and by an organization very similar to that of the holothuria : all the blood-vessels of the abdomen are made to participate in the effects of respiration. Water enters into that cavity by means of two canals, which beginning in the cloaca, end in the cavity of the peritoneum. The crocodile has an abdominal, as well as a pectoral sternum ; each sternum and its muscles regulate the effects of its peculiar and respective respiration, the pectoral sternum being called into action on land, the abdominal in water."

M. St. Hilaire ascribes the discovery of the canals of communication between the cloaca and peritoneum to MM. Isidore Geoffroy St. Hilaire and Joseph Martin, and after some reflections on the singular beauty and simplicity of this provision of nature, by which a sluggish animal becomes endowed with life and energy, he concludes, page 30, " the peritoneal canals are a species of '*trachée-artère*', to introduce the respiratory element into another and peculiar cavity adopted to the objects of respiration." We trust the preceding extracts may excite the attention of those gentlemen, and there are many in this city, who from their profound knowledge of comparative anatomy, are well fitted to illustrate the subject of respiration, and that some one of them may be induced to read before the Zoological Society, at one of its scientific meetings, a comprehensive memoir on the ordinary and extraordinary modes of effecting so important a function.*

ANATOMY AND PHYSIOLOGY.

Causes of the Presentation of the Head in Accouchement.—
M. P. Dubois read a work entitled : " A Memoir on the Cause of

* We beg to refer to Professor Grant's admirable lectures on comparative anatomy, now publishing in the *Lancet*, for some important facts connected with the respiration of the holothuria, and also of alligators, which lead him to differ from the views entertained by Geoffroy St. Hilaire.—See *Lancet*, Feb. 1st, 1834.—Ed.

the Frequency of Head Presentations, and on the Instinctive and Voluntary Movements in the Fœtus." The author first examines the question, why at the ordinary term of gestation, and even some time before, the fœtus is placed in the uterus with the head down and bent a little on the chest. He combats the opinion of the ancients, almost entirely forgotten at present, of the tumble which the fœtus was supposed to make between the seventh and eighth month. He then passes on to the opinion now generally admitted, that the weight of the head of the fœtus draws this part down, and that the insertion of the umbilical cord at a part nearer the pelvis than the head, admitting that the child is suspended by this cord, must favour the inclination of the head downwards, as happens in the heaviest scale of a balance. M. P. Dubois is not exact in saying that the weight of the head is heavy enough to produce this effect. When we immerse dead fœtuses of different ages in warm water, scil. from the fourth to the ninth month, after having placed them by means of strings in the position they hold in the uterus, the head does not gain the bottom sooner than the rest of the body. If the experiment is made in a common bathing-tub, the fall being slower in consequence of the mass of liquid, we observe all parts to descend with equal velocity, and if the fœtus be placed horizontally on the water, it preserves the position to the bottom: the back and shoulders are the parts which usually touch the bottom of the tub first. On the other hand it is supposed, that on dividing the body of the fœtus into halves, the weight of each must nearly balance; for if the brain is very large, the liver which is not less so, the intestines filled with meconium, and the bladder containing some urine, will nearly produce an equilibrium. In fine, the small capacity of the cavity of the uterus, and the small quantity of the waters must still weaken, instead of favouring, the influence attributed to the greater weight of the head of the fœtus. M. Dubois next combats the supposed suspension of the child by the cord. At two months and half, and even before that period, the cord is already longer than the fœtus and entire ovum: on this hypothesis it would be necessary that the placenta should be always inserted at the fundus of the uterus; in the cases where the cord is rolled round the child's neck, the pelvis should always present itself to the orifice of the uterus: now we know by experience that it is not so. In women who by reason of particular circumstances keep a horizontal position for almost the entire time of their pregnancy, head presentations are not less frequent than in others. Acephalous fœtuses should always present the pelvis; but it is not so; and though with them the presentation of the head is less frequent than in well-formed fœtuses, the preponderance of the pelvis is here but accessory, as we shall see farther on.

If we admit the influence of weight, we must necessarily admit that it is exercised particularly in the first months of gestation, during which the waters are more abundant, and the head of the fœtus is proportionally more developed. Now it is principally before the seventh month that the head presentations are less frequent in com-

parison to the others, as is proved by the following results obtained at the Maternité.

Children born before seven months.				Vertex.	Pelvis.	Shoulder.	
1829,	.	.	30	.	22	7	1
1830,	.	.	35	.	16	18	1
1831,	.	.	23	.	13	9	1
1832,	.	.	24	.	14	17	2
<hr/>				<hr/>	<hr/>	<hr/>	<hr/>
112				65	51	5	

The last was expelled before we could ascertain the presentation. Thus in 121 deliveries before the seventh month, sixty-five were head presentations, fifty-one by the pelvis, and five by the shoulders. The births by the pelvic extremities are then in the proportion of four to five, or of sixteen to twenty, whilst at the ordinary period of nine months, the proportion is in general one to twenty. In fine, in all mammiferous animals, whatever be the conformation of the uterus, the foetus presents almost always by the head, though the inclination of the organ towards the end of gestation is opposite to that of the uterus in woman. On examining some foetuses of cats, rabbits, &c., we immediately perceive that the predominance of the abdomen in the foetuses of quadrupeds, which has been admitted to explain this fact, is not real, and that the head in those animals is as developed as in the human foetus. From these facts the author concludes, that the position of the head of the foetus in the uterus is not at all determined by the force of gravity. He conceives that the causes of this position "reside in the want or desire which nature has impressed on the foetus, to be at a certain period of pregnancy in the situation in which it is found, and in a sort of instinctive or voluntary action, which brings it there when it is accidentally removed from it." M. Dubois enters here into the examination of the question, whether there exist instinctive or voluntary movements in foetal life. It is said that the foetus enjoys only vegetable life. The author remarks, that if it be denied that the movements of certain parts of vegetables are the result of spontaneous determinations in the individual, we cannot refuse to admit them as such in zoophytes, whose existence is purely vegetable. If the imperfection of the organs of the human foetus be alleged, he replies, that certain animals which continue foetuses a long time after birth, as the tadpole, caterpillar, &c. have, in spite of the imperfection of their foetal organization, no loss of instinctive and voluntary determinations, which it is impossible to deny: thus the tadpole swims with rapidity in pursuit of its prey; the caterpillar spins the shell in which it is enclosed.

The author, to answer the objection that the foetus, removed from external agents, receives no impressions, examines the movements of the infant enclosed in the womb, and their causes. He states, that the application of the hands to the abdomen of the mother, particularly with pressure, and the apposition of the finger to the head of

the child through the os-uteri, towards the latter part of pregnancy, or during delivery, very often give rise to well-marked movements in the latter. On the other side it is observed, that the motions of the foetus are repeated in a great number of cases in certain conditions, such as great changes in the situation of the mother, certain attitudes in the interval between meals, particularly when it is long. In fine, when during pregnancy or labour the funis is compressed, the foetus seems to indicate, by repeated movements, the feeling of constriction which it experiences, and the wish to be removed from it. If the pressure be continued, these movements are succeeded by efforts to respire, which, according to circumstances, cause air or some of the waters to enter the lungs. To all these proofs of the existence of instinctive and voluntary movements in the foetus, there are joined those furnished by the foetus of animals, such for example as the act of the bird, which when come to the term of incubation, breaks its shell to get out of the egg. As to the internal cause which excites those determinations, the author acknowledges that it is unknown to him.

M. P. Dubois then meets some objections which might be raised against this theory. The most important is this : how, if the position of the foetus in the uterus is the result of instinct, do dead foetuses present it as well as those alive ? To answer this argument we must distinguish *three epochs in pregnancy*, during which the foetus may be expelled : the first including from the commencement of the fourth to the commencement of the seventh : the second, all the entire seventh month, and the third, the eighth and ninth months. We have seen above, that living or dead foetuses expelled before the seventh month, are born almost as often by the pelvis as by the head : the objection then does not hold regarding this period ; when with respect to the foetuses who have died during the third period, it is supposed that they have had time to assume this position, and that once taken this position cannot change by reason of the closing of the uterus : yet even the death thus slow increases much the chances of presentation by the pelvis, as the following cases prove : out of ninety-six foetuses which died during the third period, and were born at the Maternité, for the last four years, seventy-two presented the head, twenty-two the pelvis, and two the shoulder. The ratio between the pelvis and head presentations was as one to three-fourths. If the foetus dies during the seventh month, there is as much chance for one presentation as the other. During these same years, out of forty-six children which died under these circumstances, twenty-one presented the head, twenty-one the pelvis, and four the shoulder. This very remarkable result becomes still more so, when compared with that of the births of living children in the same period. In fact, out of seventy-six children born alive at seven months, sixty-one came by the head, ten by the pelvis, and two by the shoulder. Whence it follows, that at the seventh month the pelvis presentations are to those of the head, as one to six, and for the dead foetus as one to one. "So that, says the author, the influence of life cannot be doubted,

and, consequently, the influence of instinctive determinations on the ordinary situation of the fœtus in the uterus."—*Archiv. Gen.* February, 1833.

On the Development of the Hair and Wool.—Academy of Sciences, sitting of the 21st October. M. Virey sends a memoir, entitled: *Physiological and Pathological Observations on the Development of the Hair and Wool, in furred and fleecy Animals; with the intention of proving that their production is favoured:*

1. By the diminution of the nervous sensibility, or by its concentration.
2. By cold, dulness, sleep, &c.
3. By a diminution of cutaneous transpiration.
4. By a weakness of the respiratory functions.
5. By relaxation, and a sedentary and idle life, under that tepid and slightly humid temperature which prolongs the hair.
6. By the disposition to albinism.
7. The fineness of the hair is greatly favoured by the tenuity of the skin, youth, short stature, and short life, whilst frequent shearing makes the wool coarse.
8. Light nourishment also favours the delicacy of the hair and wool.
9. All that prevents the loss of the animal forces, or increases their vigour, thickens their coat, and increases the density of the hair.
10. It is particularly the genital ardour, or the least possible abuse of that faculty, which ennobles the races, and which procures the richest wool.—*Revue Med.* Nov. 1833.

Action of the Heart.—There still remain for explanation two phenomena, namely, the impulse of the heart against the wall of the chest, and the double sound that is heard during its motions. The impulse has, even up to our own times, been attributed to the systole of the ventricles, and the manner of its production variously accounted for. The most simple supposition was, that the ventricles elongate themselves so as to reach the wall of the chest, which, in man, is about an inch from their apex: but it has been already proved, that they shorten themselves during their systole, and, consequently, rather retract than advance. Accordingly Senac, Hunter, and others supposed that the arch becomes straightened during its sudden repletion, in consequence of the obstruction the blood meets with, and that as it cannot move backwards on account of the spine, it presses forwards and drives the heart before it. But in the first place, as Carson has remarked, it is not true that a flexible curved tube becomes straight when injected with a fluid; in the next place, it has been proved that during the systole of the heart, the aorta is not driven towards, but from it, in the direction of the current of blood; lastly, the aorta in many animals does not form an arch at all, and yet the impulse is just as perceptible in these as in the others. Hal-

ler, having observed the apex turn up a little during the systole, in consequence of its shortening, supposed that it struck the ribs while thus bending and approaching the base; and most physiologists, Soemmering and Treviranus among the rest, adopted this opinion. But I have never seen this turning up of the apex take place to such an extent, as to allow me to attribute to it such an effect: besides, it supposes a certain position of the heart with respect to the ribs, which we do not find to obtain; indeed, such a result of the curving of the apex, would be quite impossible in most of the mammalia, in which the heart hangs perpendicularly, the base directed to the spine, and the apex, to the sternum. Lastly, Senac and Carson supposed that the apex although drawn backwards during the systole of the ventricles, was yet immediately afterwards impelled forwards and towards the ribs by the dilating auricles, and especially by the left, from its pushing against the spine. But the filling of the auricles does not take place with such a sudden shock as to cause the impulse of the heart; and besides, the latter ceases suddenly, the heart itself evidently falling backwards, while the former, which is assumed to be the cause of the forward motion, not only continues, but even increases. However this opinion, though untenable, serves to lead us to another view, to which the insufficiency of all the theories that account for the impulse of the heart by the systole of the ventricles, is of itself enough to dispose us, namely, that lately brought forward by Corrigan and Stokes, according to which the ventricles being completely filled with blood by the systole of the auricles, and brought to their greatest degree of diastole, are elongated, pushed forwards, and pressed against the ribs, from which they again retreat in consequence of their immediately succeeding systole. In order to observe directly the nature of the impulse, I have performed many vivisections on rabbits and horses; but the impetuous motions of the heart, which under such circumstances confuse the observer, and so quickly give place to the stillness of death as to demand the greatest expedition, did not permit me to attain my object entirely; and during artificial respiration after the cessation of life, the stroke of the heart in rabbits was too weak to reach the ribs. However, I have convinced myself that the apex of the heart actually moves forward during the systole of the auricles, and backwards during that of the ventricles. In like manner Stokes remarked, that when the finger was laid on the apex of the heart in rabbits, it was found to retreat at each systole of the ventricles, and to advance at each diastole: in a goat this motion amounted to two or three lines. A second ground for this view is, that the arterial pulse is not simultaneous with the impulse of the heart, but follows immediately after it. It is not very easy to attain to certainty on this point; partly because the systole of the ventricles follows so closely upon that of the auricles, that we cannot always distinguish them even in the heart when lying before our eyes; and the retrocession of the heart succeeds so quickly to the advance, that the entire appears a mere convulsive action: and partly because it requires a considerable exertion to observe accurately two

objects at once with the same sense, as the attention generally fixes itself on one of them in preference. Any one can make experiments upon himself, by feeling the impulse of the heart with the right hand, and the pulsation of the carotid, or of the right radial artery with the left; and this method has the advantage that we can choose a time when the beating of the heart is less frequent than usual, and repeat our observations without disturbance. However, it is still better to make them upon the horse, the action of the heart being less frequent on that animal: the impulse is to be felt by applying the hand to the thorax, while an assistant examines some artery, the maxillary, for instance, and marks each pulsation accurately by a noise or visible motion. In both these ways have I, as well as Corrigan and Stokes, clearly distinguished occasionally, though not constantly, the succession of the two strokes. But it is an important fact, that it was previously recognized by observers whose judgment was not influenced by any theoretic views; namely, by Soemmering, according to whom the interval amounts to two-thirds,* and also by Steinbuck. Magendie, who observed the same phenomenon, attributes the later pulsation of the arteries to the circumstance that the stroke of the heart requires a certain time for its propagation along them; but though this might cause a perceptible difference in the time of pulsation of the most remote arteries, it certainly could not in those situated very near the pulsating organ. Besides, Pigeaux has remarked of the arteries in general, that their pulse alternates with the impulse of the heart against the ribs. Lastly, Stokes has observed that the pulsation of the jugular vein, which is produced by the systole of the right auricle, is exactly synchronous with the impulse of the heart. Accordingly, we may consider it as demonstrated, that the impulse of the heart depends on the diastole of the ventricles, just as the pulse does on the diastole of the arteries.

With respect to the sounds heard on applying the ear to the region of the heart, different opinions have been entertained. Laennec attributes the first, which is comparatively duller, stronger, and of long continuance, to the systole of the ventricles; and the second, which is clearer, weaker, and shorter, to that of the auricles. Turner agrees with him as to the first sound; but thinks the systole of the auricle is either not heard at all, or else synchronously with that of the ventricles; and that the second sound proceeds from the collapse of the pericardium, or even from the diastole. Williams explains the first sound as an effect of the systole of the ventricles and auricles together; and the second as produced by the action of the valves of the heart. According to Despine, the first depends on the systole of the ventricles, and the other on their diastole. Lastly, according to Corrigan, the first sound is produced by the systole of the auricles, and the second by that of the ventricles. This opinion,

* A third is the sixtieth part of a second.

to which Stokes and Pigeaux also assent, is the only well-founded one : still it has not yet been made sufficiently clear, an object which perhaps will be more satisfactorily attained in the following elucidation.

The sound in question, as it altogether follows the type of the muscular action of the heart, must depend on that action, though not immediately ; that is to say, it cannot proceed from the striking of the parietes of the organ against the blood during the systole, as the systole of each part only ensues when that part is completely filled with blood, and there is consequently no empty space remaining, by means of which alone would the production of sound be possible. The sound can, in fact, be caused by the systole only in as far as the latter drives the blood in another cavity that is partly empty (i. e. that contains some air,) where it rushes in a stream against the parietes. While this conclusion forces itself upon us as necessary, from its very nature, it is no less confirmed by the fact, ascertained by experiment, that when air, injected into a vein, reaches the heart, the sound becomes unusually strong, and audible even to the by-standers. Nyssen compared it to the noise produced by beating up white of egg with water : and found it to be isochronous with the action of the heart ; after a short time it ceased, the air having been taken up by the blood. When Rosa and Scarpa had bled animals to exhaustion, and then restored the action of the heart by the injection of the blood of other animals, they heard likewise, without applying the ear, a great gurgling, which could only arise from the meeting of the injected blood, with an unusually great quantity of air that had collected in the heart, in consequence of the over bleeding. But the blood can cause a sound even in the vessels, when they happen to contain a certain quantity of air : thus Hertwich perceived a whizzing sound when air got into an open vein ; and in a case of varicose aneurism in the arm, where, by means of pressure, the artery and the vein could be emptied of blood, Schottin, during the re-admission of the blood, heard a noise which, according to his account, was comparatively duller, stronger, and deeper in the arteries, and higher and clearer in the veins. Kennedy has lately demonstrated by numerous observations, that the sound heard in the abdomen of a pregnant woman, corresponding with her pulse, and differing from that of the foetal heart, is actually produced in the maternal placenta, as was maintained by Kergeradec : now, as this sound is heard only during pregnancy, and that at the spot where the foetal portion of the placenta is situated, we may reasonably suppose that this respiratory organ of the foetus receives air that has been evolved in the vessels of the membranes and of the maternal placenta. But to proceed, if the sound in the heart is produced by the rushing of the blood into a cavity containing air, it must originate in the ventricles and the origins of the arterial trunks, as these are the only parts of the vascular system which are alternately empty and full, and contain both air and blood. Accordingly, we may conclude, that the first sound is produced simultaneously with the systole of the auri-

cles, by the rushing of the blood into the ventricles, where it meets with air, and forces it, before the valves have entirely closed the passage, into the origin of the arterial trunks, now partly empty in consequence of the advance of the blood; and further, that the second sound proceeds from the rushing of the blood into the arteries from the systole of the ventricles, and the consequent driving of the air there collected back again into the ventricles, now beginning to dilate anew. This conclusion is confirmed by the following facts. The rhythm of the sounds corresponds perfectly to that of the systole of the auricles and ventricles: to the first sound, which corresponds to the contraction of the former immediately succeeds the second, answering to that of the latter, and then a pause ensues, which marks the diastole. Laennec's explanation, therefore, is at variance with the most indubitable observation, and accordingly Turner and Williams supposed, that the systole of the auricles was heard at the same time with that of the ventricles. The first sound is heard at the very moment when the ear, applied to the chest, receives a push from the impulse of the heart against the ribs; and as this impulse proceeds solely from the systole of the auricles, the sound must arise from the same cause. When I applied my ear to the chest of a horse, while an assistant examined the pulse of the carotid just above the sternum, or that of the maxillary artery, on the inner surface of the jaw, and marked it by a well defined noise, I found that the second sound was synchronous with the arterial pulse, or rather, as Corrigan remarked, followed immediately after it. Should the latter be the normal form, the second sound must be caused by the meeting of the air that has been driven back from the arterial trunks into the ventricles, with the blood rushing into them out of the auricles. Stokes, in some experiments on rabbits, in which he had laid open half of the cavity of the thorax, found that the systole of the auricles was simultaneous with the first sound, and that of the ventricles with the second.—*Burdach's Physiologie*.

Extracts from the Life of Caspar Hauser.—Besides his extraordinary equestrian talents, this extreme peculiarity, the almost preternatural acuteness and intensity of his sensual perceptions, appeared particularly remarkable in Caspar Hauser, during his abode in Professor Daumer's house.

As to his sight, there existed, in respect to him, no twilight, no night, no darkness. This was first noticed by remarking, that at night he stepped every where with the greatest confidence, and that, in dark places he always refused a light when it was offered to him. He often looked with astonishment, or laughed, at persons who, in dark places for instance, when entering a house, or walking on a staircase by night, sought safety in groping their way, or in laying hold on adjacent objects. In twilight, he even saw much better than in broad daylight. Thus, after sunset, he once read the number of a house at a distance of 180 paces, which, in daylight, he would not have been able to distinguish so far off. Towards the close of

twilight, he once pointed out to his instructor a gnat that was hanging in a very distant spider's web. At a distance of certainly not less than sixty paces, he could distinguish the single berries, in a cluster of elder berries, from each other, and these berries from black currants. It has been proved by experiments carefully made, that in a perfectly dark night, he could distinguish different dark colours, such as blue and green from each other.

When at the commencement of twilight, a common eye could not distinguish more than three or four stars in the sky, he could already discern the different groups of stars, and he could distinguish the different single stars of which they were composed from each other, according to their magnitudes, and the peculiarities of their coloured light.

From the enclosure of the castle at Nuremberg, he could count a row of windows in the castle of Marloffstein, and from the castle, a row of the windows of a house, lying below the fortress of Rothenberg. His sight was as sharp in distinguishing objects near, as it was penetrating in discerning them at a distance. In dissecting plants, he noticed subtle distinctions and delicate particles, which had entirely escaped the observation of others.

Scarcely less sharp and penetrating than his sight was his hearing. When taking a walk in the fields, he once heard at a comparatively great distance, the footsteps of several persons, and he could distinguish these persons from each other by their walk. He had once an opportunity of comparing the acuteness of his hearing, with the still greater acuteness of hearing evinced by a blind man, who could distinguish even the most gentle step of a man walking barefoot. On this occasion he observed, that his hearing had formerly been much more acute; but that its acuteness had been considerably diminished since he had begun to eat meat; so that he could no longer distinguish sounds with so great a nicety as that blind man.

Of all his sense, that which was the most troublesome, which occasioned him the most painful feelings, and which made his life in the world more disagreeable to him than any other, was the sense of smelling. What to us is entirely scentless, was not so to him. The most delicate and delightful odours of flowers, for instance the rose, were perceived by him as insupportable stench, which painfully affected his nerves.

What announces itself by its smell to others, only when very near, was scented by him at a very considerable distance. Excepting the smell of bread, of fennel, of anise, and of carraway, to which he says, he had already been accustomed in his prison, for his bread was seasoned with these condiments, all kinds of smells were more or less disagreeable to him. When he was once asked, which of all other smells was most agreeable to him? He answered, "none at all." His walks and rides were often rendered very unpleasant by leading him near to flower gardens, tobacco fields, nut trees, and other plants, which affected his olfactory nerves; and he paid dearly

for his recreations in the free air, by suffering afterwards from headaches, cold sweats, and attacks of fever. He smelt tobacco, when in blossom in the fields, at the distance of fifty paces, and at more than one hundred paces, when it was hung up in bundles to dry, as is commonly the case about the houses in the villages near Nuremberg. He could distinguish apple, pear, and plum trees from each other at a considerable distance by the smell of their leaves. The different colouring materials used in the painting of walls and furniture, and in the dying of clothes, &c., the pigments with which he coloured his pictures, the ink or pencil with which he wrote, all things about him, wafted odours to his nostrils which were unpleasant and painful to him. If a chimney-sweeper walked in the streets, though at the distance of several paces from him, he turned his face, shuddering from the smell. The smell of an old cheese made him feel unwell, and affected him with vomiting. The smell of strong vinegar, though fully a yard distant from him, operated so powerfully upon the nerves of his sight and smell, as to bring the water into his eyes. When a glass of wine was filled at table, at a considerable distance from him, he complained of its disagreeable smell, and of a sensation of heat in his head. The opening of a bottle of champagne was sure to drive him from the table, or to make him sick. What we call unpleasant smells, were perceived by him with much less aversion, than many of our perfumes. The smell of fresh meat was to him the most horrible of all smells.

When Professor Daumer, in the autumn of 1828, walked with Caspar near to St. John's church-yard, in the vicinity of Nuremberg, the smell of the dead bodies, of which the professor had not the slightest perception, affected him so powerfully, that he was immediately seized with an ague, and began to shudder; the ague was soon succeeded by a feverish heat, which at length broke out into a violent perspiration, by which his linen was thoroughly wet. He afterwards said, that he had never before experienced so great a heat. When on his return, he came near to the city-gate, he said that he felt better; yet he complained that his sight had been obscured thereby. Similar effects were once experienced by him (on the 28th of September, 1828,) when he had been for a considerable time walking by the side of a tobacco field.

Magnetic and electric Phenomena.—When Professor Daumer held the north pole towards him, Caspar put his hand to the pit of his stomach, and, drawing his waistcoat in an outward direction, said that it drew him thus; and that a current of air seemed to proceed from him. The south pole affected him less powerfully; and he said that it blew upon him. Professor Daumer and Professor Herrmann made afterwards, several other experiments similar to these, and calculated to deceive him; but his feelings always told him very correctly, even though the magnet was held at a considerable distance from him, whether the north pole or the south pole was held towards him. Such experiments could not be continued long, because the

perspiration soon appeared on his forehead, and he began to feel unwell.

In respect to his sensibility of the presence of other metals, and his ability to distinguish them from each other by his feelings alone, Professor Daumer has selected a great number of facts, from which I shall select only a few.

In autumn, 1828, he once accidentally went into a shop filled with hardware, particularly with brass goods. He had scarcely entered, before he hurried out again, being affected with a violent shuddering, and saying that he felt a drawing in his whole body in all directions. A stranger who visited him, once slipped a piece of gold of the size of a kreutzer into his hand, without Caspar's being able to see it; he said immediately that he felt gold in his hand. At a time when Caspar was absent, Professor Daumer placed a gold ring, a steel and brass compass, and a silver drawing pen, under some paper, so that it was impossible for him to see what was concealed under it. Daumer directed him to move his finger over the paper without touching it; he did so, and by the difference of the sensation and strength of the attraction which these different metals caused him to feel at the points of his fingers, he accurately distinguished them all from each other, according to their respective matter and form. Once, when the physician, Dr. Osterhausen, and the royal crown-fiscal, Brunner, from Munich, happened to be present, M. Daumer led Caspar, in order to try him, to a table covered with an oil-cloth, upon which lay a sheet of paper, and desired him to say whether any metal was under it. He moved his finger over it, and then said, "There it draws!" "But, this time," replied Daumer, "you are, nevertheless, mistaken; for (withdrawing the paper) nothing lies under it." Caspar seemed, at first, to be somewhat embarrassed; but he put his finger again to the place where he thought he had felt the drawing, and assured them repeatedly, that he *there* felt a drawing. The oil-cloth was then removed, a stricter search was made, and a needle was actually found there. He described the feeling which minerals occasioned him, as a kind of drawing sensation which passed over him, accompanied, at the same time, with a chill which ascended, accordingly as the objects were different, more or less up the arm; and which was also attended with other distinctive sensations. At the same time, the veins of the hand which had been exposed to the metallic excitation, were visibly swollen. Towards the end of December, 1828, when the morbid excitability of his nerves had been almost removed, his sensibility of the influence of metallic excitatives began gradually to disappear, and was at length totally lost. Animal magnetism manifested itself in him in a manner equally surprising; and he retained his receptivity of it for a much longer time than he did that of metallic excitements. But as the phenomena which appeared in Caspar, agree in all their essential characteristics with similar appearances in other well known cases, it would be superfluous to add any other observations respecting them, than that he always called his sensation of the streaming in

upon him of the magnetic fluid, a blowing upon him. He experienced such magnetic sensations, not only when in contact with men, when they touched him with their hand, or when they, even at some distance, extended the points of their fingers towards him, &c., but also when he was in contact with animals.

When he laid his hand upon a horse, a cold sensation, as he said, went up his arm; and when he was mounted, he felt as if a draught of wind passed through his body; but these sensations went off after he had several times rode his horse around the riding school.

The Placenta probably a Gland, from Dr. Graves' Lecture on the Lymphatic System.—Fohman has proved that the lacteals always terminate in a *cul de sac*; and therefore that they absorb through the membranes which form their parietes. Müller, whose splendid work on the structure of glands I have analyzed in the first number of the Dublin Journal of Medicine, shews that the ultimate ramifications of all ducts terminate in *cul de sacs*. From this it follows that no *direct communication* exists between the vessels which secrete and the ducts which receive the secreted fluids. It is probable, therefore, that the maternal vessels and those of the foetus may carry on an active interchange of principles, although no direct communication by openings or mouths exists between them. The vessels of the mother may carry principles necessary for the nutrition and respiration of the foetus, and may freely impart them to its vascular system, while, on the other hand, every thing which requires to be excreted from the foetal system, may, in like manner, as easily find its way into the maternal veins, and so be gotten rid of. *I cannot help thinking that this view of the placenta, which, by comparing the relation between the ultimate ramifications of the maternal and foetal vessels with those of the secreting vessels and ducts in glands, points out not merely the analogy, but almost the identity of this relation, deserves the attentive consideration of physiologists. It is to Müller that we are indebted for understanding the analogy which the lungs bear in their structure to glands; and his researches first suggested to me the idea, that the placenta may be considered as a temporary gland, destined for the nutrition and respiration of the foetus.*

PATHOLOGY AND THERAPEUTICS.

Luxation of the Pubis in Labour, by Dr. Riecke.—A strong woman, 23 years old, pregnant of her first child, had a very painful labour; the child died during delivery. The last pain was so violent, that the woman was lifted up convulsively from the seat where she was to be delivered, but was retained there by assistants. Nine

days after delivery, Dr. Riecke seeing the patient for the first time, discovered a luxation of the pubis; the symphysis was broken, and the left pubis was carried behind that of the right side to the extent of half an inch at least. The external parts of generation were very much swollen, still the lochia and urine passed off freely: the patient complained of pain in the region of the symphysis, and she was wholly unable to walk, stand up, or sit down. The patient being made to lie on her back, Dr. Riecke applied his hands to the crests of the two ilia, pushing them back, endeavoured to separate them from each other as much as possible, for the purpose of restoring the natural situation of the pubis; but in consequence of the violent pain which this attempt caused the patient he was obliged to discontinue. However the same process, after several attempts, succeeded at length in the morning of the second day, whilst the patient held the legs strongly bent on the thighs. The moment the luxation was reduced, the patient was able to walk; to remedy the vacillating walk, and to strengthen the articulation, she was directed to wear for some time a bandage similar to those used in hernia. An examination of the pelvis shewed that the descending rami of the pubis formed an acute angle, a circumstance which narrowed the lower aperture.—*Heidelberger Klinische Annalen*, t. vii. 3^e cah. 1831.

Opium in Phthisis.—Dr. Berndt, Professor of Midwifery at Greifswald, on the Baltic, while treating a young man for phthisis, in the course of which he had to administer large doses of opium, remarked that his patient, after expectorating a good deal of pus, and suffering under profuse sweats and colliquative diarrhoea, not only enjoyed quieter nights, but also felt his strength increasing, and gradually recovered. This induced him to try large doses of opium, to the extent of 120 drops of a tincture of the strength of a grain in every seven drops, three or four times a day, beginning with a few drops. In the last stages of phthisis purulenta, he combines acetate of lead with the opium, beginning in like manner with small doses, such as a quarter of a grain, and proceeding by degrees to very large ones. The most severe symptoms are alleviated by this treatment, although abundant purulent sputa had been expectorated daily.

Cholera among the Insane.—At the Sitting of the Royal Academy of Medicine, 3d September, M. Ferrus read the commencement of a memoir of the progress and the effects of cholera in the hospital of Bicêtre, and on the ravages which it produced among the insane. The result was, contrary to the general opinion received, that the insane are subject like other men to epidemic influences; that the security which they enjoy does not diminish for them its severity, and that it is quite as necessary for their preservation, to withdraw from them the causes which deteriorate the organization.

M. Villermé disputed some propositions advanced by M. Ferrus, with regard to the progress of epidemics which travel, and which,

according to M. Ferrus, generally proceed from east to west; with regard to the nourishment of the prisoners of Bicêtre, which is superior to that of the poor; and lastly, with regard to the danger of cholera being in general greater to the two extreme ages, than for the middle, as the slight loss of the veterans at the hospital particularly proved, who only lost one man out of 160.

Sitting of the 10th. *Fœtus found in the abdomen of a female aged 78.* J. M. Cloquet gave to the academy for M. Mojon, the communication of a singular fact. It is of a fœtus of three months, found in the abdomen of a woman 78 years of age, in the centre of a cartilaginous tumour, which occupied the right side of the true pelvis, and adhered to the bladder, vagina, and womb, by very strong cellular tissue. This is an instance of extra-uterine peritoneal pregnancy. M. Breschet observed, that facts of this nature are very rare, and until now considered as very doubtful.

M. J. Cloquet answered, they had been well authenticated in animals: such encysted tumours have been seen in cats, which contained fœtuses fully developed, and which had no adhesion but to the peritoneum. In women, it is true, these facts are rarer and more difficult to authenticate.

M. Velpeau, on the contrary, believes that there are very positive examples. He quoted two examples, where the fœtuses had no connexion either to the ovaries, fallopian tubes, or uterus. M. Velpeau particularly quoted a woman at Port-a-Mousson.

M. Breschet contended, that having had occasion to examine that question in concert with M. Beclard, and the facts being scrupulously examined, they both considered these facts not well authenticated, and that they could come to no certain conclusion.

M. Capuron said, he and M. Lisfranc saw a case which they believed to be reversion of the uterus. The woman died, and upon examination a large tumour presented itself, which contained a fœtus seven months old.

M. Lisfranc added, that the envelope was homogeneous, and hard like cartilage. The observation was, at the time, (in 1823 or 1824,) communicated to the academy.

M. Deneux advanced, that pregnancies of that nature seldom arrived at the accustomed period, as to the rest, error in such cases is very easy. Being at Naples, he saw a female opened, who, according to the persons present, had peritoneal pregnancy. That pregnancy was in the fallopian tube, but we must not be astonished at their having published it a case of peritoneal.

M. Velpeau again rose, to recal a fact which M. Esquirol had seen, and which was analogous to the one reported by M. Mojon.

M. Esquirol affirmed to that effect, that at Salpêtrière, a tumour was found in the abdomen of a woman 68 years of age, analogous to the one in question, and which was entirely independent of the genital organs.

M. Moreau agreed both with M. J. Cloquet and M. Breschet. In woman there is often confusion on that point. Latterly he again

saw at the Maternité, in the abdomen of a woman, a foetus with its face towards the diaphragm, and which contained a kind of secondary cyst, or a false membrane, which was spread through the whole intestinal canal; but the umbilical cord was attached to a placenta incorporated with the ovary. In animals, these kind of facts are often observed; a she rabbit, some years back, contained a certain number of foetuses, floating and detached in the abdomen.

M. Lisfranc contended, that this phenomenon is as constant in woman.

Hemorrhage from between the Placenta and the Chorion; Abortion at seven Months, by Messrs. Grisolle and Caseaux.—Rocques-Marie-Joseph Herce, aged 29, pregnant for the fifth time, was seven months gone, and for nearly the last four months has lost a certain quantity of blood daily, by the vulva. These continual hemorrhages, which increased towards the sixth month of gestation, had so reduced the young woman, naturally strong, although a little of a lymphatic temperament, that she has become pale and yellowish, as if chlorotic—she has severe palpitations, is not able to work, and has syncope frequently during the day.

The 5th of May, at five o'clock in the evening, the uterine hemorrhage became more abundant, at the same time, dull inward pains, the situation of which seemed to be in the uterus, indicated an immediate abortion. The midwife who was called believed she felt the placenta at the neck of the uterus; alarmed at that circumstance she conveyed the patient to the Hotel Dieu, where we found that the soft and effaced neck of the uterus, was dilated to the size of a five franc piece; the slight distention of the membranes allowed us to determine a first position of the head. We could not feel any soft body either at the neck of the uterus, or in the neighbouring parts. All hemorrhage having ceased, and her pulse being sufficiently strong, we left her. The pains which had almost ceased, soon returned, and at four o'clock in the morning she was delivered of a seven months child. In its passage the foetus brought away with it the cord and a membranous mass, which one of us believed to be one of those membranous placentæ described by authors. Immediately afterwards, five or six clots of blood about the size of one's fist followed. The foetus was dead, and life appeared extinct only a few seconds. The skin was not oily (*circuse*), the internal organs still contained blood; but for all this we believe it died of hemorrhage; M. Orfila having proved, that when the foetus dies of hemorrhage, there is not an absolute vacuum of the vessels, nor discoloration of the viscera and muscles, as was believed. During the following night the woman expelled some more clots rather less than the former, together with a placenta of ordinary size. Its uterine surface was smooth, and quite in its natural state: the foetal surface, on the contrary, was covered with clots of blood nearly half an inch thick. The placental vessels were completely ruptured at about half an inch or an inch from the sur-

face. The other extremities of the divided vessels could be observed at the placental surface of the membranes: they were either open or obstructed by clots of fibrin newly formed. The principal divisions were in their natural state permeable to the blood. The woman experienced no accident, and of her own accord quitted the hospital six and thirty hours after delivery.

The preceding case appears to us very interesting, and we have not been able to find an analogous one in the best works on midwifery.

Vaccination, Cowpox.—Academy of Medicine, sitting of the 1st October. M. Fiard states, 1st, the opinion of Jenner, who conceives that the cowpox is given to the cow by the fluid found in the legs of the horse: 2nd, the opinion of M. Robert, who believes that the cowpox is the smallpox passed from man to the cow, and taking, when transferred to that animal, the particular form by which it is distinguished; 3rd, the opinion according to which persons consider cowpox as proper to the cow, in the same way as smallpox is proper to the human being. After which M. Fiard gives an account of some experiments made by himself to illustrate these three questions.

He inoculated cows: 1st, with the virus extracted from the legs of the horse; 2nd, with the virus of smallpox—nothing resulted from these experiments. M. Fiard was not able to repeat the experiment made in England by Sunderland. He believes that cows are often subject to a disease very much resembling cowpox, and which is still not the same disease.

To prove which, M. Girardin referred to the experiments by which Dr. Sunderland alleges that he inoculated cows with cowpox, by enveloping them with a cover containing the miasma of smallpox. Suppose the experiment efficacious, it gives us the means of removing cowpox, and proves that smallpox preceded it. The Academy had some idea of renewing the experiment, but were not able to proceed for want of funds. M. Girard has proposed to renew them at Alfort. The 15th of September last, bed-clothes which had been used by persons labouring under smallpox, were furnished by M. Piedagnel, and sent to Alfort by M. Girardin. They were carried for several days by cows without any result. In England, and in Italy, the same attempts have been made, and without happier results. M. Girard intends to recommence these experiments at Rambouillet.

M. Bosquet adds, that a lady of the department of Tarn renewed the experiment, and did not succeed.

M. Husard remarked, that the first vaccine committee had made the same experiments at Paris and Rambouillet with vaccine matter. The result was also negative.

M. Husson believes that the memoir of M. Husard is defective here. M. Boiveau Laffecteur, sen., had himself inoculated, but he insisted that the vaccine should be taken directly from a cow. He

had several cows at his house in Mouffetard-street. M. Husson vaccinated some of them—vaccination took, and served to vaccinate with success both his child and M. Boiveau Laffecteur himself, who had vesicles only on one arm. That experiment has been repeated on the cow without success.

Congenital Exstrophy of the Bladder, by M. Velpeau.—In the memoirs of the Royal Academy of Medicine, there appears a very interesting paper, by M. Velpeau, on the above defect of conformation. M. Velpeau was commissioned by the Academy to examine an account of a case, submitted to them by Dr. Mallet, of a child six days old, born at the regular term, on whom there was observed a tumour of the size of an egg, of a reddish colour, situated in the hypogastrium below the umbilicus. Inferiorly this tumour was continued in the form of a gutter over an appendage, which presented itself in front of the pubic region. Irreducible, pliant, and of a fungous appearance, it presented four nipples, from two of which there exuded a clear liquor, which had all the characters of urine. In other respects the child was perfectly healthy and strong.

This he designates by the name of *exstrophy*, or *extroversion* of the bladder. The red tumour he considers to be the posterior paries of the bladder. The inferior nipples enclose the orifice of the ureters. The appendix and groove below represent the penis cleft in its dorsal aspect, and presenting posteriorly two apertures, which were taken for the entrance of the seminal ducts. A considerable number of cases with this defect have been recorded by medical writers; the female sex, it is admitted, is not exempt from it, at the same time that the inconveniences are not near as distressing as to males for obvious reasons. With respect to the anatomical varieties accompanying this defect, cases are recorded where both the anus and penis were wanting. In a considerable number there was no umbilicus; in other cases, though the navel exists, it is found situated much lower than usual. Separation of the pubis is another coincidence, and one also which has been mentioned by Baillie in his *Morbid Anatomy*. With respect to the nature of this defect, it was for a long time considered as the result of a hernia, of reversion of the bladder. The incorrectness of this opinion was however clearly established by Temon some years back. He saw that the bladder was destroyed in all its anterior part, and that the tumour which presents is the posterior half of this viscus pushed forward by the abdominal viscera, as if to make up for the loss of substance which the sub-umbilical region seems to have undergone. Various explanations have been given to account for this defect. Dr. Duncan in the *Edinburgh Journal* attempts to shew, that *an obstacle to the expulsion of urine* satisfactorily accounts for it. Others insist on a *primitive monstrosity* in the case. The greatest number explain the phenomenon by the hypothesis of *arrest of development*.

If hypothesis be at all necessary, M. Velpeau prefers considering that the defect depends on an *alteration*, either *pathological*, or

purely physical, of the lower part of the abdomen, contracted during embryonic life. It is there that the parietes of the abdomen are longest in closing. They there remain thin and very delicate, even up to two or three months. At a later period still they acquire not the same density nor tenacity as they possess above the cord. At first this is separated from the sexual organs by so small a space, that the least cleft may become the origin of a large ulceration. Hence, then M. Velpeau concludes, that exstrophy of the bladder is *a disease*, or *the effect of a disease*, and not a monstrosity. An ulceration, a perforation of the penis or hypogastrium, is its ordinary commencement. The alteration of the bladder is but secondary. If the foetus continues to live, and that abortion does not come on, or if the ovum is not transformed into a mole, the edges of the bladder so destroyed unite to the circumference of the hole in the abdomen, or to the posterior edge of what remains of the hypogastrium. Cicatrization being effected, the remainder is at once explained by the mucous nature of the organic substance which occupies the place of the pelvic or abdominal parietes. Thus it is conceived, that the umbilicus may or may not be included in the loss of substance; that the bones of the pubis which are *destroyed* in such cases, and not merely *separated*, as is thought, may also be sometimes preserved; lastly, that the vesical tumour may occupy but the space of a few lines in some cases, and in others include a considerable portion of the hypogastrium.

SURGERY.

Torsion of the Arteries, practised with success in amputation of the leg, at the general hospital of marines at Alexandria, in Egypt, the 26th July, 1833, by Dr. Clot-Bey.—The 26th July, 1833, Dr. Grassi, in the general hospital of marines, amputated the left leg for a case of comminuted fracture, of an Arab sailor. I proposed to the operator the twisting of the arteries, and he requested me to proceed, not knowing himself how to execute that new method. Then with a common forceps, I seized successively the extremities of the anterior and the posterior tibial, the only bleeding arteries; and in drawing them towards me, I placed the thumb and index finger of the left hand above the points of the forceps; I turned them four or five times on their axis; afterwards I loosened the vessel, and pushed it slightly into the tissues. The stump was left uncovered a few minutes, and I convinced myself that the mode employed had entirely stopped the hemorrhage; but however I was not quite secure. The wound was covered and dressed, and I then applied a tourniquet, and confided it to the care of a student, so that in case of hemorrhage he might immediately stop it. It is now fourteen days since the operation, and the wound is nearly cicatrized, without any manifestation of the least flow of blood.

This is one of the first cases in which the twisting of arteries has been employed in man. It is not sufficient for us to conclude in favour of that method, but it will serve at least to encourage practitioners to try it.

It was during my last sojourn at Paris, in following the experiments of Dr. Amussat, that I understood the advantages which would ensue from the twisting of arteries: it is to the complaisance of that zealous and learned investigator that I am also indebted for having enlightened me on other important points of the science.—*Revue Med.* Nov. 1833.

Lisfranc's Views regarding Superficial Cancer.—The recent progress of pathological anatomy has shewn, that cancerous affections do not simultaneously invade all the tissues of the organ in which they are developed: thus, for example, in cancers of the stomach, the disease is sometimes limited to the muscular coat, sometimes to the cellular layer uniting it to the mucous membrane; and even when all these membranes are affected by the disease, careful dissection enables us to distinguish the tissue primarily attacked.

This regular succession in the progress of cancer had for a long time engaged my attention; in the cases of women in my hospital, whose breasts were affected with inveterate cancers, I had satisfied myself that the disease had been arrested for years by the pleura, which continued entirely free in the midst of the surrounding disease.

I had repeatedly observed the same thing in a great number of very old women, who were brought from the Salpetriere into the amphitheatre, as subjects for my lectures on operative surgery: in three subjects who had died of carcinoma in the umbilicus of very long standing, I had ascertained that the peritoneum, in the abdomen, opposed the same barrier to the advance of cancer, as the pleura did in the chest; finally, I had a slight recollection of having several times dissected in my youth, the penis which had been amputated for cancerous affections, in which I discovered that the corpus cavernosum was perfectly sound, notwithstanding the destruction of the other tissues constituting this organ.

On considering these facts, I conceived the possibility of converting to the benefit of surgery data so positive, furnished by pathological anatomy. When an organ is cancerous, may we not hope that the disease will be thus limited in its progress, especially if there enters into its structure an organic layer adapted to its insulation? And if such be the case, instead of sacrificing the entire organ, as usually directed, may we not hope to preserve it, by removing only the part really diseased? Would not such be the real end of surgery which is to preserve, not to destroy?—*Mem. Acad. Roy. Med.*, tom. trois.

These views of M. Lisfranc's are illustrated by some highly important cases.—*Ed.*

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PART I.
ORIGINAL COMMUNICATIONS.

ART. XI.—*On the Use of Mercury in Ulceration of the Cartilages of Joints.* By JAMES O'BEIRNE, M. D., Surgeon Extraordinary to the King, one of the Surgeons of the Richmond Surgical Hospital, House of Industry, Dublin, &c. &c.

(Read at the Surgical Society of Ireland, March 8th, 1834.)

EVERY experienced surgeon must have felt, that all the means which have been hitherto employed to subdue this destructive and excruciating affection of the joints, fail but too frequently in saving life or limb; and that they act always too slowly, and seldom so decidedly as to completely remove the disease, and secure the patient from a relapse. Hence it is, that Cruveilhier declares, that he knows no means of curing this ulceration, supplying the destroyed cartilages, and favouring their reproduction; and yet, that nature every day covers with new cartilages, or with new osseous vegetations, the surfaces of bones which were not destined to rub against each other.*

* Je ne connais aucun moyen de remédier à cette usure, de suppléer aux cartilages détruits, de favoriser leur reproduction, et cependant tous les jours la na-

Hence it is, also, that Mr. Brodie, in many parts of his invaluable work on diseases of the joints, is found expressing nearly similar opinions. Speaking of synovitis terminating in ulceration, he says, "no method with which I am acquainted, is capable of doing more than somewhat checking the progress, and somewhat relieving the symptoms of the complaint," p. 108, 2nd ed. Again, treating of ulceration of the cartilages as a primary affection, he states, that he is not "acquainted with any medicine which can be said to exercise a specific influence over this complaint,"—p. 177. It is manifest, therefore, that there are these all-important defects in our means of combating the disorganizing process, and the suffering which attend such cases. With a view to their removal, the subject has long occupied my attention; but how far I have succeeded remains to be shown, and to be determined by further experience.

Mercury, from its decided and happy effects in all kinds of membranous inflammation, always appeared to me to be the only agent capable of answering the various ends in view. But the injurious consequences so generally said to attend its use in scrofulous persons, who are so frequently the subjects of this affection of joints, deterred me for a long time from giving it a trial. The following considerations, however, could scarcely fail to have great weight. In the first place, it must be admitted, that injurious consequences have not always attended the use of mercury in strumous persons, and as far as my experience has led me to observe, they have supervened chiefly in cases where ptyalism has been produced slowly, or only in a slight degree. Secondly, we are not without means of anticipating and preventing the supervention of such consequences, or of combating them when they have supervened. Thirdly, in

ture couvre de cartilages nouveaux des surfaces osseuses, qui n'étaient point destinées à frotter les unes contre les autres, ou des végétations osseuses nouvelles.—*Dictionnaire de Médecine et de Chirurgie Pratiques*, t. iii. p. 509, art. Articulations, (Maladies Des.)

cases of syphilitic iritis occurring in scrofulous persons, we do not hesitate to use mercury, so as to produce ptyalism as rapidly as possible, in order to save the eye; and yet, we rarely observe serious consequences to attend the practice. Fourthly, it is manifest that there is much greater danger in permitting such a disorganizing process to go on unchecked in the cartilages of joints, than from any of the ordinary consequences of the free use of mercury in strumous habits. Ultimately, these considerations prevailed, and decided me on giving this agent a trial in cases of the description under consideration; and to employ it so as to act rapidly and fully on the mouth. The first trial was made, about a year ago, in the case of a woman admitted into the Richmond Surgical Hospital, with ulceration of the cartilages of the knee joint. In this case, the value of the mercurial plan of treatment seemed to be established by the disappearance of all the symptoms, as soon as the mouth became affected; and by the fact of the woman regaining the full power of walking, and being discharged perfectly well, in the course of a fortnight from her admission. Since that time, I have tried the same plan in similar affections of the joints, and with similar results. Three of my colleagues in the Richmond Surgical Hospital, and Mr. Cusack Roney, one of the senior surgeons of the Meath Hospital, have also employed it with success; and I shall now detail some of the cases which have been treated by myself and these gentlemen, on the mercurial plan.

WRIST JOINT.

CASE I.—James Carroll, aged 21, of delicate make, and pale, strumous countenance, admitted on the 21st of September, 1833, into the Richmond Surgical Hospital, under my care. He makes the following statement:—about four months ago, he received a blow on the right wrist, which produced pain and swelling of the joint. These were removed in the course of four weeks by stupes, poultices, and blisters. Six weeks

after, he got a fall, by which, the wrist joint was violently flexed, and immediately became painful, and soon after swelled. Leeches, cold lotions, fomentations, succeeded by blisters, and a variety of other stimulating applications, were employed by several medical gentlemen, under whose care he had been, but without any benefit. His state, on admission into this Hospital, is as follows: the right wrist joint is considerably swelled, and evidently contains a fluid. The hand, when unsupported, hangs down, and the ligaments of the joint being greatly relaxed, it may be moved much more freely than usual, and in almost every direction. On pressing the hand upwards, so as to bring the articular surfaces of the wrist into contact, excessive pain is felt in the joint; but on making extension from the hand, so as to separate these surfaces, he says, that he is relieved from pain, which is otherwise constant and severe, but so much more so at night, that he scarcely enjoys any sleep. The skin of the back of the hand approaches to a livid colour. His pulse is quick, and the tongue covered with a whitish fur. The bowels are rather regular.

On his admission leeches were applied to the wrist. Afterwards blisters were repeatedly placed on the joint, and dressed with strong mercurial ointment. Subsequently, frictions with liniment of olive oil and sulphuric acid were used; iodine lotions, and lotions of acetate of lead were applied; straps of belladonna and mercurial plaster were placed round the joint, and so as to exert considerable pressure; and the fore-arm and hand were constantly kept extended on a splint. The internal remedies employed, were purgatives occasionally; colchicum draughts at bed time; ten drops of tincture of iodine taken three times daily for a week; and compound infusion of sarsaparilla. But all these means proved fruitless; the wrist continued as much swelled as before, and the pain and sleeplessness were rather increased than diminished, while the general health and strength seemed to have suffered considerably.

Under these unpromising circumstances, I resolved on put-

ting him under the influence of mercury. Accordingly, on the 12th of October, (three weeks from his admission into the Richmond Surgical Hospital), eighteen grains of calomel and three grains of opium were ordered to be made into six pills, and he was directed to take one of these every third hour.

14th. No material change. Pills repeated.

15th. Mouth becoming affected. Slept last night better than usual; and there is now much less pain on bringing the articular surfaces in contact. Complains of griping pains in the bowels, which have been constipated for some days. Ordered, a draught of castor oil, compound tincture of senna, peppermint water, and mucilage.

16th. Severe griping pains; bowels not freed; mouth somewhat more affected; has had some sleep. Ordered to have a bolus consisting of five grains of calomel, fifteen grains of compound powder of jalap, and a sufficient quantity of electuary of senna; and if the bowels are not freed in the course of two hours, to take a draught of infusion of roses and sulphate of magnesia.

17th. Bowels well freed; no griping pains in the abdomen; mouth still more affected; general state improved.

21st. Mouth very sore. Slept soundly last night; pain remarkably diminished, and some diminution of the swelling of the wrist. Ordered a gargle composed of two ounces of the solution of chloride of soda, five ounces of water, and one ounce of simple syrup.

22nd. Ptyalism profuse. Pain and swelling of the joint still further diminished. Inside of the mouth to be brushed with a ten grain solution of nitrate of silver. A cold lotion to be applied over the joint.

23rd. Great swelling, tension, pain, and redness of the hand and inferior portion of the fore-arm, which the patient attributes to the cold lotion. The hand and fore-arm to be enveloped in a warm emollient poultice. Cold lotion to be omitted. Gargle to be repeated.

25th. Swelling, tension, redness, and pain nearly gone. Sleeps well at night. General health improved. The hand not only does not fall down when unsupported; but he has even regained the power of flexing and extending it tolerably well.

30th. Complains of nothing but weakness of the joint. Discharged from Hospital; but directed to keep the hand extended on a splint for some time, and to frequently use friction with olive oil.

This man again presented himself at the hospital on the 31st of last December, (two months from his discharge), with a small fluctuating tumour situated over the centre of the third metacarpal bone of the same hand. This tumour was opened by a lancet, and discharged a small quantity of scrofulous matter. In a few days the abscess was perfectly healed; and he was able to resume his ordinary occupations; but the ligaments of the wrist were still rather relaxed.

KNEE JOINT.

CASE II.—James Flood, aged 23, admitted into the Richmond Surgical Hospital, under my care, on the 16th of September, 1833. About eighteen months ago, he had a gonorrhea, and a swelled testicle, for which he was placed under the influence of mercury. He afterwards caught cold, his right knee swelled, and he was admitted into the Richmond Hospital by Dr. Peile, who succeeded in completely removing the swelling and other symptoms. Shortly after leaving the hospital, his left knee became attacked with a dull pain, which slowly increased in acuteness during seven or eight months, until about four months ago, when it became excessively great, and the knee began to swell. At present, except at night when it is very severe, he has no pain when lying quietly, and in the horizontal position; but when he moves, or attempts to stand, or when the heel is struck ever so lightly, he immediately feels very acute pain in the knee joint. He complains also of an acute pain extending down the outer side of the leg, and termi-

nating at the sole of the foot. The effusion into the joint is considerable; but there is no discoloration of the cutaneous surface. His general health is impaired.

Frequent leeching, cupping to eighteen ounces above the knee, blisters, lotions of acetate of lead, frictions three times a day with strong mercurial ointment, anodyne draughts, and draughts of camphor mixture and wine of colchicum, were employed in succession, but without any benefit, until the 28th of September, when he was ordered to take a pill containing three grains of calomel, and half a grain of opium, every third hour.

September 30th. Mouth not affected. No material change. Pills repeated.

October 1st. Considerable ptyalism. Pain and swelling greatly reduced; and he feels comparatively little pain in moving in bed, or when the heel is struck smartly. Pills omitted. Ordered to have a draught of vinum colchici and camphor mixture at night.

October 2nd. Complains of severe griping pains in the stomach and bowels. Ordered a purgative draught, followed by an emollient enema; a blister to be applied to the epigastrium; and when his bowels have been freely moved, to take occasionally saline effervescing draughts, and beef tea *ad libitum*.

October 3rd. Slept soundly last night. Mouth still considerably affected; griping pains gone. Pain and swelling of the knee remarkably diminished, and very little inconvenience from striking the heel; no pain whatever in the outer side of the leg. Enema, saline draughts, and nourishment repeated. The knee to be covered with an emollient poultice, frequently renewed, and as warm as he can well bear it.

In a few days, this man found that he could stand upon the limb, and walk about without pain, but in doing so, said that the joint and limb felt very weak. He was, therefore, confined to bed, ordered frictions of soap liniment to the limb, and to have full diet. Under this plan his general health recruited,

and the power of the limb became so completely restored, that he was discharged from hospital on the 26th of October, that is, on the twenty-eighth day from the commencement of the use of mercury.

CASE III.—John Ashmore, aged 15, transferred from No. 1 Ward, House of Industry, to the Richmond Surgical Hospital, and placed under my care, on the 14th of December, 1833. His intellectual faculties are rather weak ; his stature is low for his years ; and the lumbar portion of the spinal column is observed to be slightly curved towards the left side. It appears, that about two months ago, a wheelbarrow had accidentally fallen upon his right knee, and caused it to become considerably swelled and very painful. Two blisters applied to the joint, removed the swelling, but the pain continued, and instead of being confined, as at first, to the knee, shot up the thigh and down the side of the leg, and became of such a starting, lancinating kind, as to frequently awaken him from sleep. His present state is this: the right thigh is much more rigid than the left; the affected knee is of the same form and size as the sound one; when in bed, striking the right heel gives him great pain in the corresponding knee, which he is disposed to keep in the bent position. He cannot walk without feeling great pain in the affected joint, and in doing so, rests on the heel of that side, with the toes turned upwards.

17th. Ordered six pills, each composed of three grains of calomel, and half a grain of opium, and to take three of these daily, until the mouth becomes affected.

20th. Mouth not affected. No perceptible change. To have three pills as last ordered.

21st. Mouth slightly affected. States that he was less frequently disturbed last night by pains. Pills repeated as yesterday.

22nd. Mouth very little more affected than yesterday. Complains of pain in the head and back. Pulse quick, tongue white, skin hot, thirst, bowels confined. He appears to have

caught cold, in consequence of a window close to his bed having been left open for some hours. Ordered to take immediately fifteen grains of hippo and one grain of tartarized antimony, and in a few hours after it has acted as an emetic; to have a dose of infusion of roses and sulphate of magnesia, a blister to be applied to the nape of the neck, and to be placed on low diet.

23rd. Feverish symptoms abated. The emetic and the purgative mixture repeated.

24th. No pain in the head or back; pulse ninety; tongue cleaning, bowels free, but is thirsty, and the temperature of the skin is higher than natural. Ordered to be given every second hour a table-spoonful of a solution containing one grain of tartarized antimony, six ounces of water, and half an ounce of syrup of poppies. To have also a warm bath in the evening.

26th. All feverish symptoms have disappeared since yesterday, and there is now scarcely any appearance of ptyalism. Ordered to recommence the pills of calomel and opium as before, and to have middle diet.

29th. Mouth moderately affected. The starting pains are less severe and constant in the joint; he can bear more weight on the limb, in standing; and he says, that striking the heel does not hurt him so much. Calomel and opium pills repeated.

January 1st, 1834. Greater tenderness of the gums, and fetor of the breath, but little salivation. Further improvements in all the points noticed in last report. Pills repeated.

2nd. Ptyalism fully produced. No pain whatever on striking the heel with force, or in walking or standing; but the thigh of the affected side is still rigid, and he walks stiffly, and on the right heel. Ordered to be confined to bed, and to have a pint of the compound infusion of sarsaparilla daily, for some days.

15th. Discharged; and transferred to his ward in the

House of Industry, with directions to have him kept in bed, and confined to the recumbent posture for some weeks.

On visiting and examining him this day, (March the 6th), it appears that this boy has resolutely refused to be confined to bed, or prevented from walking. He has now some puffing about the patella; slight pain in walking; walks upon the right heel; and has considerable tenderness on pressure made over the spinous process of the second lumbar vertebra. It will be necessary, therefore, to re-admit him into hospital, and to direct attention not only to the knee, but also to the affection of the spinal column.

The following case came under the care of my colleague, Dr. Ephraim M'Dowel, who, with his usual liberality and kindness, transferred its treatment to me.

ANKLE JOINT.

CASE IV.—William Cromwell, a tailor, aged 21, and decidedly of a strumous constitution, admitted into the Richmond Surgical Hospital, on the 11th of February, 1834. He states, that about three years ago, he first experienced a dull pain, without any swelling, in the left ankle joint; and that in a year after, he could not sit with his legs crossed, as is usual with persons of his trade, from severe pain in the joint. A blister, applied to the ankle, was followed by an aggravation of pain, and the appearance of swelling. He was afterwards admitted into the Downpatrick Infirmary, where blisters and frictions with tartar emetic ointment were employed, but without affording him any relief. There is now such swelling as to obliterate the natural depressions about the ankle; he has severe pain in the joint, increased by striking the heel; and it is so much more severe at night, that he is frequently disturbed from sleep. He cannot walk or bear his weight upon the limb without considerable pain, and he complains of frequent startings of the leg and thigh. His general health appears to be good. Ordered to be placed on low diet, and take, every third hour, a pill, containing three grains of calomel and half a grain of opium.

16th. After taking three pills, his mouth became slightly affected, and soon after he became faint, vomited, and refused to take any more pills. Ordered to take a dessert spoonful, three times in the day, of a mixture composed of an ounce of infusion of cinchona, and one grain of oxymuriate of mercury; and to rub the joint with tartar emetic ointment.

18th. Mouth affected, but not fully; pain and swelling of the joint diminished; starting of the limb less frequent. A few scattered pustules about the joint, and on the inner side of the foot; tartar emetic ointment discontinued; mixture continued.

19th. Slept well last night. Slight salivation; feels much less pain on striking the heel. One grain of calomel, and three grains of aromatic powder, ordered to be taken every fourth hour.

20th. Has slept soundly during the night. Mouth fully affected. Pain and swelling still more diminished; can stand and bear upon the affected limb, and in doing so, complains chiefly of weakness, and very little of pain in the joint. Calomel pills omitted.

22nd. A considerable degree of salivation. Very little pain on striking the heel, but it is felt in standing, yet much less severely than at the time of his admission. When in bed he moves the joint freely, and without inconvenience, which he could not do before, and he says, that "the joint is much more supple and easy than it has been for a long time."

24th. Improving. Ordered a gargle of chloride of soda.

27th. Mouth very little affected. Sits, on being directed, in the cross-legged posture, and says, that he has far less pain in doing so, than he had felt during the last two years. Standing still gives considerable pain in the ankle; less thickening about the joint. Ordered a grain of calomel and three grains of aromatic powder, every third hour.

28th. Mouth not affected; has been very frequently purged, and with severe pain. Twelve grains of calomel, and two of

opium, to be divided into four pills, and one of these to be taken every fourth hour.

March 1st. Mouth very little affected ; has had no purging or tormina. Pills of calomel and opium repeated.

2nd. Considerable ptyalism. Gargle of chloride of soda repeated. Pills omitted.

3rd. Mouth still much affected. Able to sit cross-legged without any pain. Complains of slight tormina.

8th. Very little affection of the mouth ; appearance of the joint improved ; has very little pain in standing. Can sit cross-legged, and in this position springs up and down so as to throw the weight of his body on the ankles, without feeling any degree of pain. Thinks himself getting rapidly well. Ordered to be still confined to the recumbent posture ; to rub two drachms of camphorated mercurial ointment daily, to the affected joint ; to take a pint of the compound infusion of sarsaparilla every day ; and to be placed on full diet.

This man is still in hospital, and improving daily, but the disease has been of such long standing, that perfect recovery cannot be expected for some weeks.

HIP JOINT.

CASE V.—On the 28th of November, 1833, I was requested to see Miss Eliza O——, residing at Booterstown Avenue, near this city, aged fourteen, of an active, lively disposition, with blue eyes, and a clear, delicate complexion. This young lady's mother informed me, that her daughter was observed, about a month ago, to limp about the house, apparently without pain ; that in walking, she moved the right leg outwards, and so as to describe a half-circle ; that about a fortnight ago, she complained of a pain in the left hip, groin, and knee, which was at first of a dull kind, but gradually became so acute, particularly at night, that her father became alarmed, and confined her to bed. On carefully placing her for examination, in the horizontal position, the internal malleolus of the left leg is situated

lower by half an inch than that of the right; and the left heel evidently descends lower than the right. Striking the left heel, ever so gently, gives her great pain in the hip joint. The slightest pressure over the left groin, where there is some tumefaction, or on the corresponding trochanter, also causes severe pain in the hip joint. She complains of acute pain in the knee, shooting along the leg to the ankle; and she cannot bear the left ham to be pressed upon. Pressure along the course of the sciatic nerve produces no inconvenience. When taken out of bed, she stands with the affected knee bent and advanced beyond the sound one; and puts only the toes of the left foot to the ground; the left nates is flattened, flaccid, and apparently broader than the right, and there is no vestige of the transverse fold in which it naturally terminates. Pulse natural, general health little impaired, bowels rather confined, tongue whitish.

This being the first opportunity that I had of treating the disease in the hip, on the mercurial plan, I was naturally anxious to give it a trial. But I considered it only fair and prudent to represent all the facts of the case to the young lady's parents, and recommend a consultation. My representation had the desired effect, and it was agreed to call in the able assistance of my colleague, Mr. Richard Carmichael. Six leeches, followed by cold lotions, were ordered to be applied to the affected groin.

On the following morning she was seen, in consultation with me, by Mr. Carmichael, who at once recognized the case as one of hip disease, and concurred in the propriety of employing mercury, in conjunction with other means. Ordered to take a pill consisting of one grain of calomel, and two grains of aromatic powder, three times daily, until the mouth becomes touched; to have a blister applied behind the left great trochanter; and the vesicated surface to be dressed with tartar emetic ointment; also, to be confined to the horizontal position in bed, and to lie on a firm mattress.

December 3rd. Mouth slightly affected. Says that she

feels greatly relieved from pain; but complains of the severity of the blister; bowels still confined. Pills to be repeated until the mouth becomes more decidedly affected; and if the bowels be not moved before to-morrow morning, to have a dose of castor oil.

5th. Has taken five pills since last visit. Took also a dose of castor oil yesterday morning, which produced dark green discharges from the bowels. The gums are now tender, and the breath is fetid. There is no pain on pressure upon the groin, or in the ham, of the affected side. She no longer complains of pains in the knee or leg; the left nates is nearly as plump and firm as the right, and the transverse fold at its lower margin has reappeared, and is distinctly marked, but it is situated somewhat lower down than on the opposite side; and both lower limbs are precisely of the same length. Pills omitted; allowed weak chicken broth.

8th. No pain whatever in the groin, knee, or ham. On being placed standing on the floor, she brings both heels together, and walks without feeling the least inconvenience; scarcely any ptyalism. Ordered two grains of calomel every day, and to go on, in other respects, as before.

9th. Seen by Mr. Carmichael and myself; admitted to have little, if any, vestige of the disease. Mouth slightly affected; bowels regular. Says that she feels rather weak, yet in the absence of her attendant, has been detected in running about the room, as if she had no complaint. Ordered to have strong beef tea and chicken broth *ad libitum*; to take two grains of calomel every second day; and every vigilance to be used in keeping her confined to bed, and in the horizontal position.

17th. Scarcely any affection of the mouth. Health, in every respect good, and feels much stronger; none of the external signs of the disease visible; and on being permitted, walks about the room without feeling the least pain, or any greater weakness in the left than in the right limb. Calomel discontinued. Permitted to have moderate quantities of solid ani-

mal food, and light table beer; but still to be confined to the recumbent position.

30th. Seen by Mr. Carmichael and myself. In excellent health; insists that she is "as strong and as well as ever," and is very urgent to be allowed to get up and walk about; and, after the most careful examination, no difference whatever is observable in the lower extremities. Directed to be confined to a sofa; to have a little wine and water, and the most nourishing articles of food; and to take half a pint of the compound infusion of sarsaparilla daily, for some weeks.

After being treated thus for three weeks, this young lady was at length permitted to walk about, and return to her former pursuits. Since that time, she has had no enlargement of the lymphatic glands, nor any of the other unpleasant consequences of the use of mercury in strumous persons; and she is now, (March the first, 1834), in perfect health.

The following letter and case are from one of the most distinguished members of the profession in this country, and will, no doubt, be read with that interest and attention which any communication from him is sure to excite.

" Rutland-square, February 20th, 1834.

" MY DEAR SIR,

" Agreeable to your wishes, I send you a very brief account of the case of hip joint disease, in which the mercurial plan of treatment was attended with the most decided advantage, as it seemed to check at once the progress of the disease; an effect which is no doubt owing to the powers which the exhibition of this mineral possesses in stopping the progress of membranous inflammation. That the synovial membrane of the hip joint, and not the cartilage, is often primarily engaged in this disease, we may infer from one of the first symptoms which marks its commencement,—a fulness of the groin, depending in all probability upon the increased secretion into the joint, similar to that which we know takes place in synovitis of

the knee. You will observe, that as soon as the inflammatory symptoms had yielded to this treatment, the further exhibition of mercury was discontinued, and absolute rest, with counter-stimulants, and the exhibition of sarsaparilla, only enjoined,

“ Believe me to be yours truly,

“ RICHARD CARMICHAEL.

CASE VI.—“ On the 21st of last December, I was called to the King's County to see a young gentleman about twelve years of age, whom I found, on examination, to labour under all the symptoms of an acute attack of the first stage of morbus coxarius. There were fulness of the groin, flatness of the nates of the side affected, pain in the knee; and when the heel was struck, it gave the most acute pain in the joint of the hip. The patient could not be moved without occasioning great distress. When placed in the erect position, he rested on the toes of the affected limb advanced beyond the other, and there was an apparent lengthening of the former. Pulse rapid, great thirst, and other symptoms of high symptomatic fever were present. This boy had, I understood, a bilious fever for some time, and that the attack above described immediately followed.

“ The frequent application of leeches to the groin and hip was recommended, afterwards blisters, followed by dressings of tar-tar emetic ointment applied for half an hour daily. Mercury was ordered in small doses, so as to affect the gums, but not to an extent to produce salivation, as the boy had been greatly exhausted by previous pain and symptomatic fever. I did not see him again, as I immediately returned to Dublin, but had the following communication from Doctor Fry, of Forbane. He writes, on the 12th of January, ‘ I have much pleasure in informing you, that our patient has been progressively advancing towards recovery since I last communicated with you on the subject. The mercurial action, though not to the extent of salivation, has been with very little intermission, exerted the whole time. Digestion goes on well, which appears by his bowels

acting naturally, once every day and some days twice. Appetite very good. There is now no difference in the length of both legs, and motion in the affected one can be borne without apparent pain as formerly. We have, however, adhered to your wish in enjoining the most perfect quietness.'

"In my reply, I recommended that the mercury should now be discontinued, that an infusion of sarsaparilla in lime water, should be exhibited in such quantities as the stomach would bear without inconvenience; and that the discharge from the blistered surfaces, on the groin and behind the trochanter, should be promoted, by dressing them occasionally with tartar emetic ointment for half an hour.

"On the 2nd of February, Doctor Fry again writes, 'I have much pleasure in informing you, that Master ——— has been steadily advancing towards recovery since my last. The sarsaparilla with lime water appearing to agree remarkably well with him. His general health seems quite recovered, and all inflammatory appearances in the local affection have entirely subsided. He has occupied his new bed (Earl's) for some time. It seems admirably constructed for persons so affected, and in his particular case, has contributed much to his general comfort.' Accompanying this statement, the father of the boy adds in a postscript, 'my little fellow thinks himself so well now, that he says he could walk, if he was allowed; but, of course, this is not to be thought of as yet.' "

The following case is communicated by my friend, Mr. Cusack Roney, one of the senior surgeons of the Meath Hospital, a gentleman of very considerable experience, and who has put the practice to the severest possible test, by employing no external applications whatever, and trusting solely to the internal exhibition of mercury.

CASE VII.—"John Rice, a tanner by trade, aged 17, robust, and of a scrofulous constitution, having light blue eyes, a fair complexion, and thick lips; admitted on the 27th of November, 1833, into the Meath Hospital. The surgeon who

admitted him, directed scarification and cupping behind the trochanter of the left femur. On the first of December following, he was placed under my care, and the appearances noted were these: apparent elongation of the left lower extremity; flattening, flaccidity, and unusual breadth of the nates of the same side; no vestige of the fold formed by the nates of this side; and when standing, the knee was flexed, he stood on his toes, and with the affected limb considerably advanced beyond the sound one. He complained of severe pain in the knee and at the groin, but there was no swelling in either of these situations. When the heel was struck, he experienced acute pain in the hip joint, and he felt the same sensation when he attempted to stand or bear upon the affected limb. He said also, that he was frequently awake at night by pains, which were much more severe than those which he felt during the day. On questioning him as to those pains, he stated, that they were at first of a dull kind, and had gradually increased in intensity. His general health appeared to be little, if at all, injured.

“December 3rd. No improvement. Twelve leeches to be applied to the hip.

“6th. Still no improvement. Tartar emetic ointment to be freely applied over the whole of the affected hip.

“8th. General pustulation of the hip. Still no change for the better in the local symptoms.

“12th. Pustules nearly gone, but no apparent change. Tartar emetic ointment repeated.

“14th. No other sensible change in the local symptoms, further than feeling at ease when he does not move in bed. Seen and examined by Dr. O'Beirne, who recommended the active use of mercury, and so as to affect the mouth as quickly as possible. A scruple of calomel directed to be made into ten pills, and one of these to be taken thrice daily.

“17th. Mouth very slightly affected. Feels somewhat easier. Calomel pills repeated.

“19th. A considerable degree of salivation; slept soundly

last night ; both inferior extremities are of the same length ; he has no pain in either the knee or the groin ; the nates of the affected side is more plump ; and, contrary to the strict injunctions given to him, he has been walking about the ward, and, apparently, as if he had no complaint of the kind.

“ 20th. Has persevered in walking about the ward, and he now declares, that he is perfectly well, and insists upon being allowed to go home. Discharged ; and walked away without the least limping, or appearing to feel any pain whatever.

“ About the latter end of last February, this man again presented himself at the Meath Hospital, with all the symptoms of hip disease, in its first stage. He was admitted by one of the surgeons, who ordered moxa to be applied behind the great trochanter of the affected side. But he again proved refractory, refused to permit the application of the moxa, and left the hospital, without waiting to be discharged.”

With the exception of two or three other cases, the reports of which have been lost, the foregoing are all those in which the plan of rapidly mercurializing the system has been employed in the treatment of this affection of joints. In some of these cases, the histories show that ulceration of the articular cartilages supervened on synovitis ; and in others, that it existed as a primary and independent affection. In none of them, although the subjects were all more or less scrofulous, has any injurious effect been observed to attend or follow the practice. In all of them, the quickness with which all the symptoms invariably yielded, and ultimately disappeared, on the mouth becoming affected, is a very striking and gratifying feature of the plan employed ; particularly in the cases of morbus coxarius, on account of the external signs of the disease being more remarkable in the hip than in any other joint, and of course, more calculated to display the effects of the remedy. The case of hip disease, detailed by Mr. Roney, is, however, particularly instructive and interesting. It shows, first, that mercury, unassisted by any kind of external application whatever, is capable

of effectually checking the ulcerative process in articular cartilages ;—secondly, that no remedy, however powerful, can succeed in removing the disease, unless the patient be confined to the recumbent posture, for a time sufficient to favour the reparation of such parts of the articular cartilages as may have been destroyed by ulceration. How far this mode of treatment will enable ulcerated cartilages to be repaired by cartilage, and not, as has been observed hitherto, by a substance closely resembling the enamel of the teeth, is a point which can only be determined by time, and a favourable combination of circumstances.

It will naturally be asked, is the mercurial plan of treating ulceration of articular cartilages, entitled to be considered new? I am disposed to believe that a brief review of the various kinds of affections of joints, in which mercury has been employed, will show that it is. Mr. Brodie recommends moderate doses of mercurial preparations in cases of synovitis attacking several joints at the same time. He recommends also, the cautious use of mercurial frictions at the latter end of the treatment of synovitis not affecting more than one joint. But he says nothing of its use in the early stages of synovitis, or when it terminates in ulceration of the cartilages, or in cases where such ulceration exists as a primary and independent affection. Moreover, it does not appear that, in the cases in which he recommends mercury, his object has been to produce ptyalism. Mr. Aston Key, who has lately thrown a new light on the ulceration process in joints, advises mercurial remedies in what he calls subacute inflammation of the synovial membrane; but he makes no allusion to their use when that inflammation terminates in ulceration of the cartilages. My colleague, Dr. Ephraim M'Dowel, has employed mercury actively, and with partial success, in that very fatal form of disease, the conjunction of synovitis with periostitis. According to the Dublin Journal of Medical and Chemical Science, for last January, Dr. Ebel, surgeon of a Prussian regiment of infantry, adopting the practice of Professor Wedekind, has successfully employed corrosive sublimate

baths in rheumatic affections of the joints; but it appears that he does not employ them in scrofulous subjects, in cases where ulceration of the cartilages has taken place, or with any view to affecting either the constitution or the mouth. In the sixteenth volume of the *Encyclographie des Sciences Medicales*, will be found, extracted from the *Journal Des Connoissances Medico-Chirurgicales*, a report by Monsieur Trousseau, from which it appears that he and Monsieur Recamier, of the Hotel Dieu, have also had great success in treating acute rheumatism of the joints, by frictions of large quantities of strong mercurial ointment over the whole of the abdomen, so as to rapidly affect the mouth. We see, therefore, that all the articular affections in which this agent has been employed, have been essentially different in their nature from ulceration of the cartilages. In the course, however, of my researches on the subject, I have met with a case in which the articular cartilages may be presumed to have suffered more or less from ulceration, and in which mercury, employed in the form of friction, affected the mouth fully, and removed the disease. It will be found in Professor Wilson's very useful and interesting work on the diseases of the bones and joints, and is as follows: "In one instance indeed," says the Professor, "in an enlarged knee joint, which appeared to be of a scrofulous nature, and had taken place in a person formerly much affected with glandular swellings in the neck, and had remained nearly indolent for three or four years, upon its beginning to give pain, one drachm of the mild mercurial ointment, mixed with some camphore, was ordered to be applied to its surface, and to remain there during the night; next day a soreness of the mouth was felt, which was soon succeeded by a most violent salivation, the extent of which threatened to destroy the patient, as she had recently lain in; the swelling, however, from that single application, began to lessen, and in three months had completely disappeared: she recovered slowly of the ptyalism, and so far as I know has remained well ever since. Sir Everard Home saw this case with me about fifteen

years ago."—pp. 376, 7.—In this case, it is evident, that in ordering a drachm of mild mercurial ointment mixed with some camphor, to be applied, not rubbed, on the joint, the intention could not have been to produce even slight ptyalism; and it is equally evident from the context, as well as from the fact of the author not recommending the practice, that the ultimate disappearance of swelling of the joint was looked upon in a purely accidental light. In every point of view, therefore, it appears to me, that the mercurial plan of treating the affection of joints under consideration, has some claims to novelty. But this is a consideration of very minor importance, indeed, compared with its claims to the attention of the profession, and which will, I hope, induce others to give the plan fuller and more extensive trials. In the meantime, I shall neglect no opportunity of pursuing the treatment, and lose no time in laying before the profession a statement of the results, be they favourable or unfavourable.

I might here be expected to conclude, but there is a point intimately connected with the foregoing observations and cases, upon which it will be necessary for me to touch. It is this. I have said, that we are not without means of anticipating and preventing the supervention of the injurious effects of the use of mercury in scrofulous persons; or of combating them when they have supervened; and it cannot have escaped observation, that in the cases which I have detailed, I have preferred the use of sarsaparilla to all other means of effecting these important objects. This plant, in the forms of decoction, compound decoction, extract, and simple infusion in cold water, has obtained such character in counteracting the injurious effects of mercury, and also in diseases of the joints, that it is unnecessary to account for the preference given to its employment. But several important considerations make it necessary to explain why I have preferred the infusion in lime-water to all other preparations of the plant. It is necessary in the first place, because this preparation has not as yet been admitted into any other

Pharmacopœia than the Dublin ;—secondly, because it seems to have been first introduced extensively into practice in this country, and appears to be comparatively less known or appreciated in Great Britain than it really deserves ;—thirdly, because the preparation now generally used here, and which has been employed in the foregoing cases, is, and has long been, made according to a formula differing, in various respects, from that laid down in the Dublin Pharmacopœia, or by Dr. Copland, under the head “*Infusum Sarsaparillæ Alkalinum*,” (see his *Dictionary of Practical Medicine*, Part 1., Appendix. Formul. No. 258.) These reasons make it expedient to enter into the following brief account of the origin, progress, and peculiar claims of the preparation in question.

In the *Edinburgh Medical and Surgical Journal*, for July, 1820, (see vol. xvi. p. 473,) Mr. Battley of London, informs the editor, in a short note, that “an elegant and efficacious preparation” of sarsaparilla, is made by “infusing the entire root in lime-water.” The moment I read these few lines, the chemical principle upon which the proposed mode of preparing the plant obviously proceeded, appeared to me to be so just, that I resolved on giving the preparation a fair trial, on the first opportunity that offered. Accordingly, ‘on the 28th of August, 1820, without any formula to direct me, I ordered two ounces of sliced sarsaparilla to be infused, for twenty-four hours, on two pints of lime-water, in a glass vessel well stopped, and placed in a cold, dark, situation ; then to be strained, and the half of this infusion to be taken daily. The case in which I ordered this preparation, was that of the late Mr. Joseph S——, who laboured under secondary symptoms of syphilis, combined with the effects of the abuse of mercury. This gentleman had in vain sought, in Dublin, London, and Paris, for relief from severe nocturnal pains, nodes on the head and in other parts of the body ; he had lost all relish for food, was greatly emaciated ; and amongst various other remedies, had taken large quantities of all the preparations of sarsaparilla then known. I have only to add, that after

taking a pint of the preparation above described, every day for six weeks, he was completely freed from all his complaints, regained, and continued to enjoy, a tolerable degree of health and strength, until the last year, when he died of structural disease of the heart and liver. He, however, and many others, in whose cases I had tried this infusion, complained of its taste being exceedingly bitter and nauseous; and to obviate this objection, it was ordered to be taken mixed with an equal quantity of new milk. Some could, and others could not, take it in this form. In several cases also, I found it did not act as energetically or certainly as I had reason to expect. Under these circumstances, I decided on doubling the quantity of sarsaparilla, and adding liquorice root; and the result has been, an infusion so singularly efficacious, and so agreeable, that it soon found its way into both private and hospital practice, and was afterwards recommended in strong terms by my colleague, Mr. Carmichael, in his well known work on the venereal disease. And shortly after the last edition of this work appeared, it was admitted into the Dublin Pharmacopœia, but under a very different formula; in fact, the first, and not the improved formula, which I had adopted. During nearly fourteen years I have employed the preparation which I have described, and have seen it employed by others, extensively, and with the most decided advantage, in syphilitic and mercurial affections, which had obstinately resisted all the other preparations of sarsaparilla. I have found it a powerful auxiliary in the treatment of nervous diseases; and have used it with manifest benefit in cases of debility and loss of health from various causes; also in scrofulous enlargements of the lymphatic glands; and in irritability and chronic catarrh of the urinary bladder. In one case, in particular, a case of catarrh of the urinary bladder, attended by profuse muco-purulent discharge and great debility, various remedies, and amongst others, large quantities of lime-water, were employed without making any impression on the disease, until this preparation was used, when the discharge ceased in

the course of ten days, and soon after the gentleman rapidly regained strength and health. Its beneficial effects are seldom observed earlier than about the third week of its use; and the kidneys are the only organs upon which it appears to exert any sensible action.

Having premised these facts, I shall now insert the formula for preparing and using this infusion of sarsaparilla. It is as follows:

℞. Radicis Sarsaparillæ Jamaicensis concisæ uncias quatuor.

—— *Glycyrrhizæ semunciam.*

Aquæ Calcis libras duas.

Macera per horas viginti quatuor in vase vitreo optimè operculato, et in loco frigido et obscuro; dein cola in usum.

Sumat hujusce infusi dimidium, partitis vicibus, quotidie.

This formula differs from that of the Dublin Pharmacopœia, first, in containing double the quantity of sarsaparilla; secondly, in containing liquorice root; thirdly, in requiring the infusion to proceed for twenty-four, instead of twelve hours; fourthly, in not requiring agitation of the contents of the vessel. This formula also differs from that given by Dr. Copland, first, in containing double the quantity of sarsaparilla; secondly, in not requiring the sarsaparilla to be bruised; thirdly, in not requiring agitation of the contents of the vessel; fourthly, in requiring the glass vessel to be very well, instead of slightly stoppered. There is but one of these differences that I have not satisfactorily accounted for, and it respects the time allowed for the infusion by the Dublin Pharmacopœia. Experience has long convinced me, and the following experiment, which I have repeatedly seen tried, will, I think, convince others, that that time is by one half too short to complete the process of infusion. The experiment was conducted thus, and with the following results. A stream of carbonic acid gas was directed into a quantity of the compound infusion of sarsaparilla, which had been prepared in twelve hours, when the liquid, from being clear, became quite turbid, and a white precipitate was

observed collecting at the bottom of the glass. A stream of carbonic acid gas was next directed into a quantity of the same preparation made in twenty-four hours, but the liquid remained as clear as before, and no precipitate could be observed. This is an extremely interesting fact, and shows, that the small quantity of lime contained in the lime-water, enters into intimate combination with the active principles of the sarsaparilla and the liquorice; and with them forms a new substance, which is perfectly soluble in cold water. Dr. Anthony Todd Thomson, in commenting upon the Dublin formula for this preparation, says, that "the active principle of the sarsaparilla is soluble in cold water," and that, "the use of the lime-water is not satisfactorily explained." There can be no doubt, that the active principle of sarsaparilla is soluble in cold water, but I doubt much that it is so to the same extent as in cold lime-water; and my reason for thinking thus, is this. Several years ago, I requested Mr. Daly, of Henry-street, in this city, who first made the preparation, to supply me with some of the sarsaparilla which had been subjected to infusion for twenty-four hours in lime-water. Upon this, after adding liquorice root, I had fresh lime-water poured, the glass vessel well stopped, and left to infuse for the time just mentioned. Thus prepared, this second infusion was administered, for some weeks, to several patients in the Charitable Infirmary, Jervis-street, of which I was then one of the surgeons, but it was found to be totally inert and useless. Hence it appears that lime-water takes up the whole of the active principle of sarsaparilla. All these facts will, I trust, satisfactorily explain its use in preparing this plant, and exhibit the superior powers of the preparation, into the composition of which it enters.

To conclude, it appears to me that the name given to this preparation, by Dr. Copland, is more applicable and expressive, than that given to it by the Dublin College.

ART. XII.—*A Case of Pneumo-thorax from Perforation*.

By RICHARD POOLE, Esq., Assistant Surgeon, 32nd Regiment.

PRIVATE J. ROWE, 32nd regiment, admitted into hospital at Tralee, March, 27th, 1832. He was a healthy looking lad on joining the regiment about six months ago; there was, however, slight malformation of the chest, the subclavian regions being shallow, the submammary cylindrical or bulging. Had an attack of pneumonia two months ago, which was treated actively, but the patient was for some time afterwards troubled with a bronchial affection. Present complaint, violent cough, dyspnoea, with thin, viscid, mucous expectoration of a pale straw colour, and pretty copious; no decided loss of resonance, nor any morbid sound detected in the chest, except occasional mucous rales in the lower left lobe. The disease was considered bronchitic, and treated in the first instance, with antimonials, and then with the balsams; under this plan, with milk diet, and counter-irritation, he improved very much, and regained in a great measure, a healthy look, presenting only some acceleration of pulse, occasional morning cough with scanty expectoration, and slight dyspnoea.

On the 27th April, the lower portion of the right side gave a dull sound on percussion, and here the respiratory murmur became indistinct, but no aggravation of the symptoms took place until the beginning of the month following, when the cough became again troublesome, and he began to look pale and unhealthy; the pulse was still quick, and heat of skin towards evening considerable. On the 12th May loud bronchial breathing was detected in the mammary region of the left lung, with bronchophony, but no ronchus, the resonance continuing unimpaired. It was inferred that some dilatation of the bronchial tubes existed. On the 21st a remarkable change took place,

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the patient having been attacked with pneumonia of the lower part of the left lung, which yielded to general bleeding and large doses of the tartar emetic, not, however, before hepatization had taken place to some extent. After the signs of this latter lesion had entirely disappeared, a mucous rale persisted through the whole of the posterior portions of the lung, the anterior part giving a clear sound on percussion, until the 1st July, when the metallic tinkle was distinctly heard, and a diagnosis of pneumothorax made, although the respiratory murmur and a deep gurgling could be distinguished in the portions of the lung, over which percussion elicited the clearest sound. There never existed any signs of an excavation, but the patient coughed up, on the 4th and 5th of the month, a large quantity of a sero-albuminous fluid, proving that a communication existed between the lung and the sac of the pleura. From this time he sank rapidly, and expired on the 6th of the month.

Autopsy, twelve Hours after Death.—Considerable marasmus ; left side of the chest, at the inferior portion, much bulged out, highly sonorous, in fact, tympanitic ; right side gave generally a dull sound on percussion : on cutting through the cartilages of the left ribs, a rush of inodorous gas took place from the pleural cavity. The lung was found condensed, and pressed up towards the spine and back part of the ribs, to which it was bound by strong adhesions. A small quantity of a yellow coloured serosity existed at the bottom of the cavity. The surface of the lung was covered with several loose layers of false membrane, of a lemon colour and tough consistence ; they were readily detached from the lung ; the lung was firm and corrugated, it was perforated by a considerable opening, which existed at the back part of the upper portion of the lower lobe, close to the adhesion with the costal pleura ; the finger could be passed by this opening into the substance of the lung, and it was found that a small quantity of purulent matter escaped by it when the lung was pressed. The parenchyma of this lobe was thickly infiltrated with tubercular matter in a crude

state ; one or two points of suppuration were observed, but anteriorly there was no excavation. On the back part of the lobe, however, corresponding with the situation of the fistulous opening, existed a considerable irregular cavity, which had through this discharged its contents into the pleural sac. The upper lobe of this lung was quite sound, and did not contain a single tubercle, but all its bronchial tubes were much dilated, their lining membrane highly vascular, without any apparent hypertrophy. The sac of the right lung contained some serosity, but the lung itself, with the exception of its lower lobe, which was thickly studded with crude tubercles, was sound and crepitous.

Observations.—This case was clearly one of pneumothorax from perforation, the atmospheric air having escaped into the pleural sac by means of the fistulous opening that existed in the back part of the left lung. So far there is nothing uncommon ; but from such a case it may be readily shewn, how considerable reparation might be effected in the diseased parts, provided life could be sustained so long, by which, not merely might a stop be put to further disorganization, but also all traces of the original mischief be completely obscured. It is, in fact, only necessary to allow the possibility of the atmospheric air that escapes into the pleural sac producing, as in the present instance, a pleuritis with membranous exudation, some layers of which are deposited over the fistulous opening, and prevent the further entrance of the atmospheric air ; to show that this reparation may be effected, and to establish a rational explanation of some forms of pneumothorax, at present attributed to causes much less readily assignable : we can easily understand how, under such circumstances, all traces of the fistulous opening may be completely lost, and how, after the lapse probably of years, we should in such a case discover nothing but a lung contracted and bound up into a certain part of the thorax by strong cellular bands, while the other portion of the pleural cavity contained nothing but inodorous gas. Such views are not altogether hypothetical, for I can only explain in this way a case I ob-

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served about four years ago, in which, the patient, a healthy young lad, dying from epilepsy, I was afforded an opportunity of seeing in the right side of the chest, the appearances described above. In this case, there was no contraction of the side, nor do I see, explaining it as I have now done, how such could have taken place.

In the case of Rowe, the fistula, certainly, remained open to the last, but there are several cases on record in which a fistula of the lungs has been closed. In addition to these, there are other circumstances I need not detail here, which may lead us to differ with Laennec in attributing to secretion, (exhalation), or the decomposition and putrefaction of the pleural effused fluids, the existence, in several of his cases, and particularly those in which pulmonary excavations were found, of gas in the cavity of the pleura. Andral's views on this point are evidently at issue with those of Laennec, for he states expressly, that in almost all instances pneumo-thorax takes place from perforation. It appears to me also, that the second case recorded by Dr. Graves, in the last number but one of this Journal, stands in a very doubtful light, and I am far from concurring in the view he has taken of the impossibility of atmospheric air, introduced through a fistulous opening, exerting pressure on a lung sufficient to render it in a condition that might be termed "*carnefied*." It is known, however, that Laennec has restricted, with what justice I am not going to question here, this term to the inflammation of the lung under pressure. Now, if the state of the lung in Dr. Graves' case resembled this, I can readily appreciate the value of the observation it has given rise to, but if the term is applied in this instance simply to express a condition of the lung characterized by compression, and absence of all crepitation, I have only to state, that in the case of Rowe the lung was but little crepitous, and had undergone condensation to such an extent, as to have become remarkably shrunk, and as it were wrinkled, from the compression alone of the atmospheric air. I have, therefore, to repeat, that we should hesi-

sitate to admit this case as an example of pneumo-thorax from secretion (exhalation). The other case detailed by Dr. Graves appears, however, an admirable example of this interesting lesion, and corroborates, in some measure, the views of M. Gendrin, that an exhalation or secretion of air is frequent in connexion with pleuritis. Gendrin appears, however, to attribute this phenomenon to active rather than mild examples of this disease. Dr. Graves' case appertains to the latter.

ART. XIII.—*Surgical Report of Cases, treated in the Meath Hospital during the past Year.* By WILLIAM HENRY PORTER, Lecturer on Anatomy and Surgery in the School of Anatomy, Medicine, and Surgery, Park-street, Dublin.

THE general practice of all hospitals must be nearly the same, and as the leading characters and treatment of the ordinary forms of disease are pretty accurately understood, very detailed reports are neither necessary nor instructive, unless when adduced for the purpose of establishing some important pathological fact, or introducing some improvement in practice. But in every establishment of this kind, particular cases will occasionally occur, not only novel in their nature, and therefore curious, but by reason of their infrequency, difficult and uncertain in their management. By the publication of such cases, in an authentic form, the hospital surgeon may confer the greatest benefit on his profession, for he enables the practitioner, of extensive opportunities, who has met with similar cases, to compare the observation and experience of others with his own, and thereby approach the truth; whilst to the younger practitioner, he furnishes a guide and assistance in the difficulties of his profession, which, though far from perfect, may nevertheless be valuable. In this spirit and with this view I have selected the following cases, each possessing its own peculiar interest, and on which I

forbear to offer comment or observation, wishing to render the details as short as shall be consistent with clearness ; and knowing that the case, which to the practitioner at the bed side will appear rare, or difficult, uncertain, or important, may to the reader in his closet, deprived of the numerous aids derived from personal inspection, seem to possess little more than ordinary interest, and unworthy of being obtruded on the profession.

1. Aneurism occasioned by the sequestrum, in a case of necrosis of the tibia.

2. Functional derangement of the brain, the result of injury, cured by the operation of the trepan.

3. Curious and interesting case of bronchotomy.

4. Disease of the lymphatics of the left arm, amputation at the shoulder joint.

CASE I.—*Aneurism in a Case of Necrosis.—Death from Mortification and Hemorrhage.—Post Mortem Appearances.*

John Jackson, æt. 29, of delicate habits, and had been a hard drinker, admitted January 2nd, 1833.

History.—He stated that several years previously (perhaps fourteen or fifteen) he had been seized with a violent pain in the left knee, which, as well as the lower part of the thigh, shortly afterwards swelled to a great size, but without redness. This tumefaction subsided a little under the use of blisters, but the knee always remained larger than that of the opposite limb. About a year afterwards a small swelling appeared four or five inches above the knee, on the inside, which he opened himself, and gave exit to some purulent matter ; a fistulous opening has remained there discharging ever since. In the month of August, 1832, he had a very alarming hemorrhage from this fistulous opening, but there was no recurrence of it until the night before his admission into hospital, when he bled with great violence ; the blood, at intervals, spirting forth to a considerable distance, at others trickling down the limb, but in neither case was he able to restrain it. He supposed himself to have lost

several quarts, and fainted from exhaustion seven or eight times.

Symptoms on admission.—His face appeared quite blanched and expressive of the greatest anxiety ; extreme exhaustion ; thirst ; pulse small and thrilling, 150. On removing the sheet in which the limb was wrapped, a small, livid, fistulous opening was seen on the outside of the lower third of the right thigh, slowly discharging a thin, serous blood, on pressing which the finger seemed to sink into a deep cavity ; pulsation was quite distinct, and bruit de soufflet audible for some distance round it, as if from aneurism : the femur, at its lower third, could be felt enlarged, and the popliteal space filled up, but the pulsation of the artery below it was distinctly perceptible. The weakness of the patient prevented any farther or more accurate examination, and it seemed to be a case of popliteal aneurism complicated with diseased bone ; the sac having probably burst into the cavity of the abscess in connexion with the bone. He complained of intense pain in the knee and throughout the tumour.

A compress of lint was put over the opening, with a bandage rolled from the foot, over and above it. Lemon juice *ad libitum*.

Jan. 3rd. There was some bleeding during the night, which oozed through the compress and bandage. He was this morning seized with severe vomiting. Pulse 150, full and hard.

4th. Vomited all night. This morning intense pain in the thigh ; face quite bleached with a yellowish tinge, and an indescribable expression of anxiety ; thirst very urgent ; pulse 142. In consultation amputation was decided on as offering the only possible chance, however slender, but he steadily refused to submit, although warned that a few hours would render the adoption of this only remedy impossible. He had effervescing draughts, with tincture of opium.

5th. Symptoms nearly the same, with increasing debility.

6th. No cessation of the vomiting ; great pain in the limb ; the thigh above the bandage greatly swollen and yellowish ; on its posterior surface gangrenous nearly as high as the buttock ; immediately under the compress a patch of sphacelus. The

vomiting continued, and a constant, though feeble hemorrhage, trickled from the limb. He gradually became weaker, and died about nine o'clock, P. M.

Dissection.—On opening the popliteal space, it was found filled with thick grumous clots, extending up as high as the lower third of the femur, in contact anteriorly with the bone, and with something that appeared to be part of the sac, but whether of an aneurismal sac, or the cyst of a former abscess, could not be determined. An opening existed in the popliteal artery, a little below the spot where it enters the space. The thigh bone was found diseased in its lower half, being considerably enlarged, its surface rough, and a large portion of the posterior or popliteal aspect destroyed, so as to permit the introduction of the fingers into a large cavity within; the edges of the bone on each side of this opening were thick and very full of rough sharp points; in the upper part of the excavation the sharp point of a sequestrum was discovered, moveable, and accurately corresponding to the aperture in the artery, which it evidently seemed to have occasioned. The knee joint filled with a yellowish serum, unlike ordinary synovia; its capsular ligament thickened. The cellular tissue of the entire thigh filled with a reddish serum.

CASE II.—Fracture of the Os-frontis.—Symptoms of Idiocy and Paralysis, supervening after some interval.—Operation of the Trepan.—Slow but ultimately perfect Recovery.

Edward Hughes, æt. 35, a strong healthy countryman, admitted into hospital June 26th, 1833; he had been returning from a fair on the evening of the 8th May previously, and in a scuffle with some persons who attempted to rob him, received a blow of a large stone nearly in the centre of the forehead, which knocked him down, and caused a depressed fracture of the os frontis. He was not rendered senseless by the blow, nor for some time afterwards was there any perceptible consequence, as during the six subsequent weeks he was able to work, and had his intellects perfect and natural. His friends then observed him to become drowsy, listless, and incoherent:

when undisturbed he was quite idiotic, but when roused he appeared to possess some memory. His manner of answering a question was very remarkable; he hesitated, seemed to recollect with difficulty, and answered as if in doubt. He tottered in his gait, and had a remarkable tremor in his left arm and hand: the tongue, when protruded, was drawn to the left, and there was also strabismus of the left eye.

Considering that the subsequent symptoms had some direct connexion with the injury of the skull, I determined on exposing and examining the bone, and being then governed by circumstances. I found an irregular fracture of nearly an inch in length, one side of which was depressed to the depth of little more than three lines. The trephine was then applied, in order to permit of the elevation of the depressed bone, but the internal table was found to have been so extensively broken, that three crowns of the instrument were removed before all the fragments could be exposed and taken away; one large portion had penetrated the dura mater, and entered the substance of the brain, the removal of which was followed by considerable hemorrhage, appearing to come from some vessel of the brain itself, and which could only be controlled by the application of several compresses, and such a degree of pressure as evidently affected the functions of the organ. He bore the operation tolerably well, and continued during the remainder of the day listless and in a half sleep; pulse slow, small, and weak.

June 29th. (The day after the operation.) Had remained tranquil during the night, and turned himself from one side to the other in the bed; but to external objects was nearly insensible; his evacuations passed involuntarily; pulse weak and labouring. On the removal of the compresses and dressings he became more lively, and his pulse more full.

30th. His evacuations are still passed unconsciously; tongue covered with a dark fur in the centre; pulse 86, and strong; no pain in the head; can swallow well, and appears much more sensible. He lies in a kind of slumber, but is very easily roused.

July 1st. No pain in the head ; pulse 110. This day, for the first time, he became conscious of his natural wants.

It is unnecessary to detail the treatment which was strictly and actively antiphlogistic, or the symptoms, which were those of progressive improvement until the 16th July, when he had several severe rigors, and trembled violently ; pulse rapid and fluttering ; eyes peculiarly wild ; he is drowsy and stupid.

18th. Pulse small, weak, and very irregular. He is quite unconscious of his natural wants, and appears nearly idiotic. The tremors are very remarkable, particularly of the left arm and leg, which he cannot keep quiet for a moment by any effort. The bottom of the wound is covered with white granulations resembling fungus, and the discharge very profuse, and rather fetid. He was ordered calomel and opium with a view to affect the mouth.

28th. In proportion as the effects of the mercury became developed, the patient's health improved, and on this day he might be pronounced quite convalescent. The trembling of the limbs has totally disappeared, and all pain removed ; he is, however, very weak.

Aug. 10th. He is now completely recovered, and is perfectly rational ; he walks about as strong as ever, and assists in doing several things about the hospital : he wears a large plate over the wound, which is nearly healed. He remembers most of the circumstances that occurred during his illness.

This man was able, in the November following, to walk to Dublin, a distance of twenty-three miles. He called on me, and expressed himself as being in as good health as he had been at any period previous to the accident.

CASE III.—Case of Bronchotomy, for the removal of a Foreign Body, supposed to be in the Larynx. Relief not obtained. Difficulty of determining the Nature of the Case. Subsequent Recovery imperfect.

James M'Mahon, æt. 14, hitherto very healthy, admitted into hospital August 27th. In the previous June, whilst eating

some beef-hash, a piece of bone or gristle seemed to have stopped in his throat, and he was instantly seized with all the symptoms of suffocation, violent cough, &c. He remained in this state for some days, with great pain and difficulty of swallowing, and of turning the head to the left side. He got some medicines from his mother, but being still unrelieved, he was brought to an hospital, where a probang was passed down the œsophagus with some 'slight temporary benefit. In a short time, however, the symptoms returned with increased violence, and he was then recommended by a clergyman for admission into the Meath Hospital.

Symptoms on Admission.—Deglutition so difficult and painful as to make him refrain from drinking, although very thirsty ; he cannot turn his head to the left side without great suffering ; voice nearly lost ; breathing loud and sibilous ; the wheezing greatly increased during the spasmodic paroxysms, which are very frequent. The face is pale and livid, the lips purplish. He opens his mouth badly, but as far as the condition of the fauces can be ascertained, there are no traces of inflammation. On passing the finger into the fauces, the epiglottis can be felt of its natural size, and healthy. Pulse rapid ; skin hot and dry. The cause of all this distress appeared to be very obscure, but as the commencement of the disease seemed traceable to some accident in the process of deglutition, and rendered it probable that some foreign body was impacted in the ventricle of the larynx, I proposed the operation, to which the boy's mother for two days refused to consent. At the end of this period, the symptoms having become aggravated, the operation was performed.

This was one of the most difficult and embarrassing operations in which I had ever been engaged, the annoyance having been principally caused by the first incision having been accidentally made a little to the right of the mesial line of the neck. Some large veins were thus exposed that could not be completely avoided, and the hemorrhage was so great as not only to ob-

secure a view of the parts, but to threaten danger from the quantity of blood lost. The trachea was extremely small, and was difficult to fix, and when after some delay it was opened, a quantity of blood rushing into the trachea rendered the patient's condition very precarious: violent cough succeeded, with respiration quick, difficult, and laborious. The larynx and trachea moved rapidly up and down the neck, and although after the opening had been cleared, a great quantity of thick mucus was expelled, I had never seen an operation by which the patient seemed to be less relieved.

During the afternoon and throughout the night the breathing continued very laborious, frequently obstructed by mucus, which required to be constantly removed. Whenever the canula became foul, or the air passage from any other cause interfered with, a paroxysm of suffocation supervened, and he appeared at the point of death. No sleep during the night.

August 30th. He appears much easier, and will occasionally lie for half an hour or more perfectly tranquil, but the moment the wound is in the slightest degree obstructed, he falls into a paroxysm of suffocation, and works and struggles almost convulsively. The wound is somewhat swollen, and he cannot endure the presence of the tube, but on its withdrawal, as it is difficult by any contrivance to keep the edges open and free, a fresh paroxysm of difficult breathing is produced. Two pupils sit at his bed side night and day, and it sometimes requires all their exertions to keep the wound sufficiently open, and save him when attacked by one of these paroxysms. Ordered small doses of tartarized antimony.

31st. Had a very severe attack from the accumulation of mucus, and the closure of the wound, which was relieved by the forcible dilatation of its lips by means of forceps. An abominable fetor exhaled from the wound, like that observed in cases of abscess and ulceration of the larynx. Some slight stethoscopic indications of bronchitis, but the sound of the respiration through the wound is heard all over the chest, and renders every other indistinct.

September 6th. He has been gradually improving since the last report. Very large tubes have been obtained, and two or three of them lie by his bedside, that one may be always ready, clean, and free from obstruction. Still nothing had been accomplished by the operation. The moment a finger was laid on the orifice of the tube, respiration stopped: if withdrawn for an instant the convulsive breathing was renewed, and it became evident, that the obstruction (whatever it might be) was situated above the wound, and probably in the immediate neighbourhood of the rima glottidis. I resolved on making a close examination of the parts, which I did by means of eye and probe without discovering any thing; but still dissatisfied, I passed a large sized bougie from the wound through the rima, and out by the mouth. The instrument passed with facility from above and from below, and not the slightest indication of the existence of any mechanical obstruction could be observed. Yet the larynx remained perfectly incapable of performing its function of transmitting air to the lungs.

For several days afterwards, I passed the bougie without the least good effect. He continued gradually improving in health and strength, and the paroxysms of suffocation occurred less frequently, as he had acquired some dexterity in introducing the tube for himself, and thus anticipating any serious attack. I find, however, that occasionally he incurred considerable danger when the mucus accumulated during sleep, and in the night of the eleventh such an occurrence had nearly deprived him of life. All the functions, circulation, digestion, &c. regular and natural.

16th. He sleeps now nearly the entire night through: is able to take out the tube and clean it himself, therefore, no longer requiring attendance as formerly: he is up and walking about the wards, but not the least symptom of air passing even in the smallest quantity through the rima.

This patient was retained in Hospital for more than six weeks subsequently, with the double purpose of watching

whether some foreign substance might not be accidentally expelled, and thus throw some light upon the case, and lest some sudden and severe paroxysm, attacking him when removed from professional assistance, should prove fatal. During this period he was seized with acute bronchitis, in consequence of exposure to cold, for which he underwent a full course of calomel and opium, and recovered, but still the affection of the larynx continued: not a particle of air passed through the rima, and he left the hospital with every probability of being obliged to wear the tube, and exist by artificial respiration during the remainder of his days.

During the night of the 21st December, in consequence of the tube being nearly closed up by inspissated mucus, he was seized with a dreadful paroxysm of suffocation, and must have perished but for the timely assistance of Mr. Crampton, who was called to him. On the following day he was re-admitted into hospital, more for the purpose of receiving occasional relief in these paroxysms than in the hope of any permanent benefit. Here he again suffered from an acute attack of bronchitis, for which he was treated as before, and recovered. Towards the latter end of January, he unexpectedly found, that on closing the tube, a small quantity of air passed by the larynx, and he could speak in a feeble tone, but sufficiently distinct to be understood. The process of natural respiration seemed slowly and gradually about to be restored, and occasionally he could remain with the tube closed for half an hour or more. On the 28th January he left the hospital.

I have lately seen this patient, and though greatly improved he still wears the tube, nor would he dare to sleep without it; there is, however, every reason to believe, that he will shortly be able to dispense with it altogether, and the function of the larynx be completely recovered.

The cause of the obstructed respiration was never satisfactorily discovered; nor was there any reasonable conjecture offered on the subject. At the period when it was at its great-

est height, the bougie passed with the greatest facility through the rima, which, as well as all the adjacent parts, seemed to be perfectly healthy.

CASE IV.—*Case of Disease of the Lymphatics of the Arm. Amputation at the Shoulder Joint.*

Michael Hughes, ætat. 29, of a scrofulous and emaciated appearance, admitted on the 27th November, 1833, for an affection of his left arm of a very peculiar nature.

The limb is semiflexed and slightly swollen at the elbow, the hand bent downwards at the wrist; the knuckles bent also, and every one of these joints permanently rigid. There is an eruption on the hand, which is itchy and troublesome. The veins of the arm swollen and prominent, but not hard or knotted. The pulse at the wrist so weak as to be scarcely perceptible.

An irregular line of small tumours, resembling tubercles, extends along the arm from the axilla in which the largest is situated; they are of a stony hardness, and firmly attached to the adjacent structures; some of them are ulcerated, others surrounded by a dark blush of inflammation. The pain he experiences is dreadful, and deprives him entirely of rest. He entreats that something may be done for him, and will submit to any operation that holds out even a chance of relief. The history of this case is shortly this. The tumour in the axilla was the first formed, and appeared four years previously, after a day of very hard work; its growth was slow and irregular, sometimes increasing, sometimes diminishing, and never very painful. The gland next below it then swelled, and thus the disease continued to spread along the arm in the direction contrary to the course of absorption. Several months previously he had been in hospital, under the care of my friend Doctor Graves, and was treated with iodine, and (I believe) every other medicine that offered even a chance of success, but without any material benefit. At that period I was consulted on the case, and considering the tumour in the axilla to be the chief, as well as

the original cause of the mischief, hinted, that nothing short of its removal could prove serviceable, although the possibility of such an operation was very questionable, as it seemed evident, that the great axillary vessels were closely connected with the diseased gland; perhaps enveloped by it. He heard some whispering about an operation, and quitted the hospital without leave.

During his absence he applied corrosive sublimate to the tumours, for the purpose (as he expressed it) of cutting them away, and produced some frightfully irritable ulcers. The pain now became so intolerable that he applied to me, and was again admitted into hospital.

Nothing offered any prospect of relief but amputation at the shoulder joint, nor was this exceedingly promising, as he had a slight cough, and observed his expectoration to be triflingly streaked with blood. His chest was minutely examined: it sounded well on percussion, and the respiratory murmur healthy and distinct. It appeared that nothing directly contra-indicating the operation could be proved to exist, and as he solicited that something should be done, it was determined in consultation that the part should be removed.

The operation was performed in the usual way, by making a flap of the deltoid muscle, the tumour in the axilla rendering any other mode of proceeding impossible. In removing the limb, the tumour was divided, and it was discovered that the artery, vein, and plexus of nerves were encompassed by it, a circumstance that rendered the securing of the vessel difficult. A temporary ligature of safety was then placed round all, and the tumour dissected out, the arteries tied, and the flap laid down, and secured by a few stitches. The patient bore the operation very well, but having lost a considerable quantity of blood, became weak, and was almost sinking, his pulse feeble and tremulous, his voice faint, and his limbs cold. Warm wine and water was administered, and jars of hot water to his extremities. A full opiate was ordered, but immediately rejected from his sto-

mach. Altogether his condition was most precarious, until towards evening, when he rallied a good deal, and from that moment every thing, so far as the wound was concerned went on favourably, the ligatures came away at the usual periods, and at the end of five weeks he was recovered so far as to be able to leave the hospital. But the ultimate safety of this patient is very questionable. About three weeks after the operation he was seized with cough and difficulty of breathing, amounting to orthopnoea, he expectorated a considerable quantity of blood, had nocturnal perspirations, and lost flesh, for which symptoms he was removed to the country as soon as the condition of the wound permitted.

I have since heard of him very frequently, and he continues pretty nearly in the same situation, with cough, and the other pectoral symptoms. Although the direction of the disease externally was in a line contrary to that of absorption, it seems not improbable, that the bronchial glands had also become engaged, and of course the present symptoms are not much under the control of medical treatment.

Although unrelieved by the operation, the case appears interesting, as offering an example of a disease of the lymphatic glands, (as far as I know), not hitherto observed or described. The dissection of the arm exhibited a number of these glands indurated, others ulcerated, but all exhibiting proofs of their malignant nature. Several glands, however, were found healthy, and without any alteration of structure. One in the axilla, close to the large original tumour, was slightly increased in size, and softened in consistence. The artery, vein, and nerves, had passed through the centre of the indurated gland, and the former vessel had been so compressed, that its calibre scarcely equalled that of a small crow-quill. The axillary vein was also compressed, whilst the cephalic was increased in size. Having passed the tumour, the artery again seemed to expand into its natural size, but still the compressed state of the vessel was indicated before the operation, by the smallness and feebleness of

the pulse. The distention of the superficial veins showed, that the circulation through the deeper trunks was more or less impeded. The crookening of the fingers, the rigidity of the joints, and the itchy eruption, probably had some connexion with the pressure exercised by the tumour on the nerves. True, our knowledge of the pathology of these important organs is still so imperfect, that this opinion can have little more weight than mere conjecture, but in three cases of subclavian aneurism, which I had under my care at different times, I observed a similar eruption and crookening of the fingers, which were removed after operation, when the pressure of the tumours was no longer exercised. In these latter cases the joints were certainly not rigid, but then the pressure had only existed a few weeks, whereas in the case of Hughes, it had been in operation four years. These considerations on the effect of pressure on the nerves, appear to me important, for it is only by collecting facts and comparing them with each other, and with the symptoms apparently resulting from them, that any thing approaching to sound pathological knowledge can be established.

I had intended to have added some other cases to this report, as having occurred within the past year; amongst them that of the man whose face was nearly eaten off by a pig, but independent of the length to which these details have been already drawn out, I find, that the interest of this case would not compensate for the disgust its perusal would occasion. It merely tended to shew, first, that a man deprived in this rough manner of his nose, both cheeks, both lips, and part of both ears, might recover notwithstanding; and secondly, it exemplified the uses of the lips to the functions of speech, mastication, and deglutition. It was remarkable, that soon after the accident, he could pronounce the labial letters tolerably well, a power which he lost as the wound progressed in healing, and of which he was entirely deprived before he left the hospital. His articulation of course was very indistinct. He lost more than half of the solids he attempted to chew, for want of the lips to keep

them under the teeth, and a considerable portion of the fluids escaped in his attempts to swallow. His chief regret was the being deprived of the use of tobacco, which he was incapable of using in any form, on account of the loss of his nose and lips. All the saliva secreted by the parotid glands escaped, and several attempts were made to ascertain the quantity produced in a given time, but they were defeated by the irritability of the patient. Although deprived of so large a portion of this fluid, I did not find that the process of digestion was at all impaired.

ART. XIV.—*A Case of Trial for Poisoning by Arsenic.* By THOMAS EDWARD BEATTY, M. D. M. R. I. A., Professor of Medical Jurisprudence to the Royal College of Surgeons in Ireland, Accoucheur to the New Lying-in Hospital, and Consulting Accoucheur to the Baggot-street Hospital, Dublin.

THERE is no part of medical practice, wide as that field is, which more imperiously demands not only a thorough acquaintance with all the particulars relating to it, but also a readiness in making that knowledge available when opportunity requires, than those cases in which we are called on to give an opinion respecting the administration of poison. The difficulty varies in degree according to the circumstances of the case. If we see the patient during life, and have an opportunity of witnessing the symptoms under which he labours; if some remains of the poisonous dose be forthcoming, or if the contents of the stomach, as ejected by vomiting, be preserved; and if after death we have an opportunity of examining the body, much of the obscurity is obviated, and the case is one of comparative simplicity. The difficulty is increased, when the sufferer is dead before we see him, when we can only learn the history of his previous state from ignorant by-standers; and when none of the evacuations

from the stomach or intestines are available for the purpose of analysis. But the case becomes involved in still greater obscurity when a body is found dead, without any information as to the previous sufferings of the individual ; for we are then cut off from all knowledge of the symptoms, and the means of comparing them with the appearances found on dissection ; a most important help towards arriving at a just conclusion. It is cases of this latter kind, which place a medical man in the most trying situation, and demand all the knowledge of toxicology which he can bring to the task. It behoves him to be well acquainted with the morbid effects of the different poisons, and the *variations* to which these effects are liable, according to the dose, and the rapidity with which it is followed by death. Thus, for instance, it is generally known, that one of the usual effects of the irritant poisons is inflammation of the gastrointestinal mucous membrane, terminating in a remarkable thickening and corrugation of this tunic, the prominent parts of the elevations being of a deep black colour ; yet, a person who would expect to find such appearances in every case of poisoning with arsenic, would be very likely to overlook some of the most important cases of death from that mineral ; there being cases, and these of the most deadly kind, in which little or no traces of inflammation are to be found in the stomach or intestines.

These observations have suggested themselves in reference to an important medico-legal case, lately tried in this city, in which a woman and her paramour were convicted, and executed for the murder of the husband of the former. The case is important, inasmuch as conviction was had upon circumstantial evidence, but that of a nature so clear, that there was no resisting its force.

The unfortunate victim was an elderly delicate man, with a humped back, named Thomas Canning, who kept a huxter's shop in Upper Stephen's-street, to which he had not removed many weeks, and was supposed to have about seventy pounds in

the savings' bank. He had the ground floor of the house, consisting of a small shop in front, and a small room at the back of it, communicating with the shop by a door, in which room he and his wife lived and slept. Besides these apartments, they had another on the first floor, in which they had beds to let to lodgers. The only lodger they had since they took the concern, was the prisoner Martin, who was a married man, but had long deserted his wife and children. He had lived with them in their former lodgings in South King-street for four months.

Having, through the kindness of one of the counsel for the crown, obtained a copy of the depositions sworn to before the trial, and also his notes of the cross examination of the witnesses at the trial, I have been enabled, with the help of the reports given in the public prints, to collect the principal facts, upon which this extraordinary case rested, and to arrange them in a connected form.

To take the evidence in chronological order, we have first the testimony of John Reilly, a police constable, which shews, that there had been an illicit intercourse between the prisoners, Maria Canning and Patrick Martin, for a length of time, and that so far back as the year 1832, they were in the habit of beating, and otherwise ill-treating the deceased, Thomas Canning. Reilly swore, that in December 1832, Canning came to the police office to which he is attached, to complain of abuse and beating by Martin, and his wife, who had been out all night together. On going to his lodgings, he found there Martin and Mrs. Canning. Being asked why they abused Canning, Martin denied having done so, but Mrs. Canning exclaimed, "a bloody end to the cripple, I will be his end." Canning had taken up a poker, to shew witness how his wife had struck him, when she ran forward, snatched it out of his hand, and made a blow at him, which Reilly prevented, by seizing the poker, and twisting it out of her hand. She then ran at her husband, and gave him several blows with her fist, swearing "she would have his life," that "he was no man."

They were then both held to bail for their good behaviour. Such scenes were frequently repeated, and the deceased called on Reilly several times afterwards, to complain of assaults made on him by the prisoners. The last occasion was so recent as the 7th of January, about a week before his death, when he lodged informations against Martin; at which time he bore several marks of violence on his head.

Michael Geoghegan, a lodger in the same house with the Cannings, stated, that on the 2nd of January last, Mrs. Canning, who had absented herself for three days, returned home. On this occasion her husband was very angry, and asked her, "how dare she come back after being out with any fellow," to which she only replied in a sneering manner, and asked him for a glass of spirits. In about an hour afterwards, Geoghegan heard Martin conversing with Mr. and Mrs. Canning, and they all three drank together. In some time after, he heard loud snoring in the room, and on looking through a small window in the door, he saw Canning on the floor asleep, and Mrs. Canning and Martin standing in the shop, at a press where Canning kept his bank book, and heard them disputing about eleven pounds, which she accused him of stealing. He denied it with dreadful curses, and called her a whore, several times. She replied, "you are a gentleman, but if I am a whore to you itself, you are not to rob us of our hard earnings." The next morning, the 3rd of January, they both disappeared; Mrs. Canning did not return until the night of Sunday, the 5th January, and Geoghegan heard Martin's voice in the house the next morning. Martin was in and out of the house, as a lodger, from that time until Canning's death, during which period he assumed great authority in the house, and frequently used gross and opprobrious language to Mrs. Canning; who also used gross epithets to her husband. Early on the morning of Wednesday, the 15th of January, the day on which the fatal deed was committed, Geoghegan heard Canning call his wife down stairs, from Martin's room, where she was in bed, and heard

her say to him, "go along, God damn you, or I will knock you down stairs;" upon which the deceased retired quietly. This witness saw Canning in his own room, with his wife, about seven o'clock in the evening, in his usual health, on his feet, and sober. From that time until past 12 o'clock at night, when Canning was found dead, there is no account of what passed in his room.

Mary Geoghegan, wife of the last witness, deposed, that about 12 o'clock, on the night in question, Mrs. Canning came up to her room, where she was in bed, and gave her a glass of spirits. Witness shortly afterwards followed her down stairs, to light a candle, as an excuse to get another glass. When she got into the deceased's room, the only persons she saw were the prisoners. They sent her out for more spirits, of which she procured half a pint, and they all sat down to drink it. After they had drank part of it, Mrs. Canning said, she would give some of it to her poor Thomas, meaning her husband, who was lying in the bed, with the curtains close drawn. Martin said, "hell to him if you do, he will be jawing till morning." She said she did not care, she would give him a sup, and rose up from the table with a heavy sigh, and went over to the bed, but brought no drink with her. She put her hand into the bed, and immediately cried out, "he is as cold as clay." Witness then took a candle over to the bed, and on drawing the curtain, found the deceased lying on his back, with all his clothes on, his cravat tied loosely round his neck, and his mouth wide open. She at once said he was dead, on which Mrs. Canning observed, that he was in the habit of sleeping very soundly; when witness replied, he was then in the sleep of death. She immediately ran up stairs, and awoke her husband, who came down, and found the deceased as above described. He proposed to go for Mr. Singleton, a neighbouring apothecary, to which the prisoners objected, saying, that as he was dead, there was no use in making a fuss about it. Nevertheless Mrs. Geoghegan went of her own accord, and brought Mr. Singleton in with her.

Mr. Singleton testified, that he was called out by the last witness, at about half past one o'clock on the morning of the 16th of January, and brought into Canning's room. He there found the deceased as already mentioned, and thought he must have been dead some time. On inquiring as to the cause of his death, he was told by Martin, that the deceased had been drinking during the day, that he had lain down on the bed for some time, being tipsy, and that when they went to awake him, they found him dead. Witness observed, that if that was the case, an examination of the body would disclose the cause, and proposed to have it opened, to which Martin at once said in a very strong manner, "Oh, Sir, he shall not be opened, we will not allow it;" and in such language persevered in his objections, which created suspicion in the witness's mind. He asked if the deceased had made any noise before he died to alarm them, and was told not. He was not then informed of his having vomited. Witness then went home, and after he had gone Mary Geoghegan stripped the body and washed it, in the presence of the prisoners, and in doing so did not perceive any thing remarkable on it. Martin then went out for more spirits, and he and Mrs. Canning continued drinking till morning; they drank about a quart altogether. Mr. Singleton returned at ten o'clock in the morning, and then asked Mrs. Canning, "what noise was that which your husband made in his throat when dying;" to which she replied, "no noise, Sir, he only complained of a pain in his belly." Mr. Singleton then removed the bed clothes, and observed, as he thought, an appearance of eccymosis on the skin under the clavicles.

Mr. Thomas Weston, shoemaker, the landlord of the house, but not residing in it, on being informed of Canning's death, inquired into the particulars respecting it, and having been aware of the misconduct of the prisoners, and of their ill treatment of the deceased, he at once declared his suspicion of foul play, and with a most praiseworthy promptitude, insisted that an inquest should be held on the body. The inquest was

held accordingly in the course of the day, before Alderman Perrin, one of the city coroners. During the investigation, Mr. Singleton asked Martin, if the deceased had vomited prior to his death, who then said he had ; and when Mr. Singleton remarked that he had not seen any appearance of it about the bed, he brought him into the room, and pointed out a wet spot on the floor, to which Mr. Singleton and Dr. Coote stooped down and smelled, and thought they perceived a strong smell of opium.

Dr. Coote opened the body, and stated that he found no marks of external violence upon it. On opening the stomach, the villous coat appeared in a state of active inflammation, it had a corrugated or wrinkled appearance, and he thought it had lately contained opium, from its strong smell of that drug. He removed the stomach, with its contents, from the body, put all up carefully in a bladder, and brought it to me the following day.

The evidence which I gave on the trial was to the following effect. On Friday, the 17th of January, 1834, Dr. Coote, accompanied by Mr. Welsh, a medical student, came to my house and informed me, that he wished to ask my opinion and advice respecting the stomach of a man, who had died under suspicious circumstances, on the 15th, and whose body he had examined before the coroner's jury on the 16th. He produced a bladder carefully tied up, which he opened in my presence, and discharged the contents of it, consisting of a human stomach and heart, into a clean basin. The stomach had been opened by an incision five inches long, on its anterior surface, near to the great extremity. Some of the contents escaped into the basin, but the greater part remained. On examining the stomach, it was found to contain about half a pint of thick reddish coloured fluid, smelling strongly of anniseed, and several pieces of unchewed half boiled potato. The parietes of the organ were entire, excepting the incision already mentioned. The mucous lining presented a general red blush, and a minute

injection of the small vessels with red blood in several places, particularly in the great end. It was somewhat thickened, and some corrugations were evident at the posterior surface. In some places large patches, some of them of the size of a half crown, of white matter were found adhering to the mucous surface. These could be scraped off without difficulty, and presented the appearance of a fine white powder, mixed up with mucus into a paste, but the membrane underneath them did not appear more vascular, or more altered in texture than in other situations.* On looking at the fluid contents of the stomach, a similar white powder was perceived floating through it. The heart was quite healthy.

Having made this cursory examination, I replaced the stomach, with all its contents, in the bladder, and accompanied by Dr. Coote and Mr. Welsh, proceeded to my laboratory in the College of Surgeons. I there, as before, poured the contents of the bladder into a clean basin, and emptied the stomach entirely, inverting it, to get a clearer view of its inner coat. I scraped off a small portion of one of the white patches which still adhered, with a watch glass, and divided the little mass into two parts, one of which I transferred to another watch glass. Into one of these I poured distilled water until it was nearly full, and on stirring it with a glass rod, the white substance separated into a powder, which sank to the bottom, and a viscid substance in which it had been entangled. I now applied the heat of a spirit lamp, and cautiously boiled the fluid, by which the powder was dissolved. The clear fluid was now poured off, and divided into four watch glasses, which I arranged on a white porcelain slab. Into one of these I dropped cautiously from a tube, the ammoniacal nitrate of silver, when a copious yellow precipitate took place. Into the second I dropped the ammoniacal sulphate of copper, which gave an equally abundant precipitate of a grass green colour. To the

* The stomach is preserved in the Museum of the College of Surgeons.

third I added a little of the hydro-sulphate of ammonia, when the fluid assumed a deep straw yellow colour, which, on the addition of a drop of acetic acid, and being gently heated over the spirit lamp, gave a copious lemon yellow precipitate. To the fourth a large quantity of lime-water was added, from which there fell down a white cloudy precipitate.

Having thus satisfied myself of the presence of arsenious acid in solution, I proceeded to treat the remaining portion of the white paste, which I had set aside in its watch glass, by the reduction process. Previous to doing so, I poured the fluid contents of the stomach out of the vessel in which it had been standing, into another, and in the bottom of the former, I perceived some portions of white powder, of which I collected a few particles on the point of a spatula, and transferred them to the glass containing the white paste. Having mixed them, I thought the substance too moist to introduce into the reduction tube, so I held the glass for a short time over the spirit lamp, taking care not to apply a heat sufficient to sublime arsenic. The mass being thus gently dried, was mixed with an equal portion of black flux, which formed a ball about the size of a garden pea. I dropped this to the bottom of a thin glass tube, six inches long, one-fourth of an inch in diameter, and closed at the bottom. A paper gutter was now passed down the tube, and through it a portion of black flux was poured, so as to cover the little ball at the bottom; which done, the gutter was withdrawn. The part of the tube containing the mass was now held at a distance over the flame of the spirit lamp, so as to expel the moisture remaining in the mixture, by a gentle heat. This rose in vapour, and collected on the inside of the tube, from whence it was removed by a roll of blotting paper passed down the tube, and kept revolving between the finger and thumb, as long as the fluid continued to rise. As soon as this ceased, the roll of paper was withdrawn. The tube was now brought nearer to the flame, which was at first directed to the upper part of the contents, and by degrees the part containing

the bulk of the mass was brought into the centre of the flame, where it was held steady. In a short time a metallic ring commenced to form on the inside of the tube, about a quarter of an inch above the upper part of the black mass; this gradually increased in breadth, and at last became a quarter of an inch broad: a dark coloured ring was formed higher up in the tube, evidently composed of carbon. The outer surface of the metallic ring, in contact with the glass, was uniformly smooth, bright, and of the colour of polished steel; the inner surface appeared crystalline. This experiment satisfied me that I had obtained metallic arsenic from the substance treated.

The stomach was now well washed with distilled water, all the adhering matter was removed, the viscid contents were diluted with distilled water, and the whole was introduced into two flasks, the fluid part into one, and the solid matter, with some of the fluid, into the other, and set aside for further analysis.

On the 20th I resumed the investigation. Being anxious to ascertain the quantity of arsenic contained in the stomach, I proceeded in the following manner. To the flask containing the solid matter, six ounces of distilled water were added, and after acidulating it with acetic acid, it was boiled for a few minutes; it was then poured off, leaving the solid matter behind, and filtered while hot; a tolerably light clear coloured fluid passed through, which was collected in a deep, narrow jar, and through it a stream of sulphuretted hydrogen gas was passed for half an hour; this caused a very copious precipitate of a golden yellow colour (orpiment): the fluid was now boiled again, which caused the precipitate to become more distinct, and this was now separated by filtration and preserved. Fresh distilled water was now poured on the solid contents remaining in the flask, and subjected to the same treatment, by which an abundance of orpiment was obtained. This process was repeated four times with the same result, and the whole quantity of orpiment ob-

tained weighed, when dry, thirty-one grains, which is equivalent to twenty-five grains of arsenious acid.*

It having been supposed that laudanum had been administered to the deceased, I was desirous of ascertaining whether any of it could be discovered in the fluid contents of the stomach, which I had set aside in the other flask. For this purpose, having consulted with my colleague, Dr. Apjohn, we agreed that the arsenic contained in solution should be removed previous to any other steps being taken. Accordingly the fluid was removed to a deep narrow jar, and acidulated with acetic acid; a stream of sulphuretted hydrogen gas was now passed through it for an hour, giving rise to an abundant yellow precipitate, which was removed by filtration. The matter caught on the filter was bulky, and of a less bright colour than that obtained in the former process, owing to the presence of animal matter, which had not been separated by previous filtration; owing to this adulteration of the orpiment I did not preserve it, but the quantity was as great as that already obtained. The fluid which passed through the filter was of a light brown colour, clear, and amounting to eight ounces; this was slowly evaporated by gentle heat on a sand bath, until the residue became a brown viscid extract, having the appearance and smell of oymazome. Upon this one ounce of alcohol was digested, with heat, for five minutes, during which time it was kept stirred with a glass rod; the alcohol was then poured off into another dish, and evaporated, leaving a small portion of brownish viscid matter behind, to which was added one ounce of boiling distilled water, in which the extract readily dissolved.

* The composition of the two substances is as follows :

	Arsenic.	Oxygen.	
Arsenious acid	1 atom 38	+ 1½ atoms 12	= 50
	Arsenic.	Sulphur.	
Orpiment	1 atom 38	+ 1½ atoms 24	= 62

Hence sixty-two parts of orpiment are equivalent to fifty parts of arsenious acid, and consequently thirty-one parts of the former are equivalent to twenty-five of the latter.

This watery solution was now treated with a view to detect the components of opium, in the following manner :—A small portion of it was transferred to a watch glass, and some recently prepared mucilage of starch was well mixed with it, a drop of iodic acid was let fall into it, when the whole assumed a deep prussian blue colour.* The remainder of the fluid was then treated with a solution of acetate of lead, which caused a creamy, brownish white precipitate. After filtering the fluid, this precipitate was removed to a watch glass, a little distilled water was poured over it, and a drop of sulphuric acid was added, which changed the colour of the precipitate to a perfect white. This was allowed to subside, and the supernatant fluid was

* This test, which was first proposed by Serullas in the *Journal de Chimie et de Physique*, t. xliii., p. 211, has not been taken notice of by English writers, as far as I know, although it is mentioned by Berzelius, t. v., p. 132, and also in the *Journal de Pharmacie*, t. xvi., p. 206. The words of Serullas are these :—
 “ Si l'on met en contact a la temperature ordinaire, de l'acide iodique dissous, avec un seul grain de morphine ou d'acetate de cette base, la liqueur se colore fortement en rouge brun, et il s'exhale une odour tres vive d'iode : La centième partie d'un grain d'acetate de morphine suffit pour produire cet effet d'une maniere encore tres sensible. L'action est tres prompte, si la liqueur est un peu concentrée, elle est plus lente quand elle est etendue, mais elle n'est pas moins appréciable au bout de quelques instans, même dans sept milles parties d'eau. La quinine, la cinchonine, la veratrine, la picrotoxine, la narcotine, la strychnine, et la bruchine soumises aux memes eprouves, n'agissent aucunement sur l'acide iodique.

“ Pour rendre plus apparent l'iode mis en liberté dans l'experience, on peut commencer par triturer avec un peu de gelée d'amidon, la petite quantite de liquide contenant la morphine, ou ses sels, et on y ajoute quelques gouttes de la dissolution d'acide iodique, qui developpe aussitot la couleur bleue. Ce moyen peut servir également a reconnaitre l'opium dans ses preparations, car quelques gouttes de laudanum ou d'une dissolution aqueuse d'opium mêlées à de la colle d'amidon puis à de la dissolution d'acide iodique donnent aussitot la couleur bleue.”

It is true that with this test I obtained indication of the presence of morphine, but having completely failed with all the others, and not having had sufficient experience of the truth and delicacy of its operation, I was unwilling to place complete reliance upon the result of the experiment.

poured off, to which a drop of permuriate of iron was added, without any change of colour being effected. The original fluid from which the brownish white precipitate just mentioned had been separated, was now subjected to a stream of sulphuretted hydrogen gas, with a view to remove any lead which remained after the first part of the process. The sulphuret of lead thus formed was separated by filtration, and to the fluid which passed through the filter, ammonia was added, without causing any precipitate. However on being allowed to stand in a small test tube for twenty-four hours, a minute quantity of a greyish white deposit was formed, to which, after the fluid was removed, a drop of permuriate of iron was added, without causing any change.

From these experiments I testified at the trial, that arsenic was contained in the stomach in quantity sufficient to have destroyed life ; but I was not able to certify respecting the administration of laudanum.

Mr. Thomas A. Evans, assistant to Mr. Simon Kelly, druggist, No. 64, South Great George's-street, identified the prisoner Martin as having come to the shop on the morning of Wednesday, the 15th of January, the day of the murder, to ask the price of forty drops of laudanum. On being told the price was one penny, he purchased four penny worth, (160 drops,) saying it was for a sick woman in the country. In about two or three hours, Martin returned to the shop, saying he had lost, or broken the bottle, and purchased a similar quantity. On the evening of the same day, he came back, and asked witness for some poison for rats, when he sold him an ounce of arsenic, having added a few drops of oil of anniseed to it, to induce the rats to take it. At a later hour on the same evening, the prisoner again called on witness and said, "that," alluding to the arsenic, "had killed five rats," and he requested to have a similar quantity, which witness gave him. Witness identified an empty ounce phial bearing the label of

Mr. Kelly's shop, and marked "laudanum," as one of those in which he sold that drug to Martin."

It is to be remarked, that Weston, the landlord, and the two Geoghegans, all deposed that they never saw rats in the house ; neither were there any rat holes to be seen on the premises.

Catherine Curran stated, that she lived next door to where the Cannings resided: between 10 and 11 o'clock, of the night of the murder, she heard a noise in the back yard like the falling of a bottle, and on going out, she found a bottle lying unbroken under the kitchen window, which was the same as that identified by the last witness.

Jane Mooney, niece to Mrs. Anne Byrne, a publican, living in South King-street, in whose house the Cannings and Martin had lodged for four months previous to their removal to Stephen's-street, stated, that Martin called at the shop on the morning after Canning's death, and told her "the little man was dead," meaning Canning: witness asked of what complaint he had died, when his answer was, that they had all been drinking together, that the deceased went to bed tipsy, and turned on his face and was smothered.

Here the case for the prosecution closed. The prisoners called no witnesses, and counsel made no defence. The jury retired, and in half an hour brought in a verdict of guilty against both prisoners.

Here was a case substantiated on as complete a chain of strong circumstantial evidence, as ever was offered to a jury,—circumstantial evidence, as the learned judge (Jebb), who charged the jury, observed, more to be depended on than any direct evidence could be. The previous long continued adultery of the prisoners ; their constant abuse and ill treatment of the unfortunate deceased ; the frequent threats of murder made by his wife ; the purchase of a large quantity of poison by Martin on the very day of the murder ; the arsenic being marked by the oil of anniseed ; the deceased being seen alive and well in his own room at an early hour of the evening ; no one but

the prisoners being in his company from that time until his death ; the conduct of the prisoners at the time of the discovery of his death ; their refusal to have medical aid called in ; and their refusal to permit an examination of the body when a medical man was brought ; their conduct through the night after ; their denial that the deceased had given any warning of his death, or signs of being ill ; and the admission afterwards of Martin, that he had vomited, and of Mrs. Canning that he had complained of pain in the belly ; the finding of the body on the back, while Martin stated that he had turned on his face and smothered ; and, lastly, the finding of about a drachm of arsenic in the stomach, the contents of which smelled strongly of anniseed, furnish a connected body of such damning evidence, as left no doubt on the minds of the jury of the guilt of both.

After the trial I was very desirous of getting some accurate information from the prisoners respecting the poisoning, and through the kindness of an official friend, I sent some written questions to them, which I hoped to have had answered, viz. : Did Canning get laudanum, if so, when, and how much ? How much arsenic did he get, and how was it administered ? What effects did it produce ? Did it cause pain or vomiting ; if so, how soon and how much ? When did he die ? What symptoms preceded and attended his death ? Was there any violence used towards him ? These were all very important questions, and satisfactory answers to them would have made this a much more complete medico-legal document, but I regret to say, that Martin, who readily confessed his guilt in general terms, refused to give any particular information. He admitted that the female culprit was equally guilty with himself in every step ; that he had bought the poison for her, but denied that he had administered it. The woman remained obdurate to the last, and refused to make any confession.

I proposed the last question, respecting the employment of violence, in order to satisfy myself as to the correctness of a suggestion I had made to the crown counsel at the trial, viz.

that the culprits not finding the poison to act as speedily as they wished, might have expedited the death of their victim by some species of smothering or "burking." Not having seen the body myself, I was, of course, unable to form any opinion upon the subject, but an observation of Martin's, although he would give no positive information, seems to give a sort of negative authority for such a supposition, for he gave the gentleman who questioned him reason to understand, that Canning did not *die* of the poison. This is, however, as I have mentioned, but negative authority for the truth of such a presumption; and it is a subject upon which we must now, of course, remain in total ignorance.

Before I close this account, I think it right to call the attention of the reader to a species of poisoning with arsenic, I believe not very generally known, and of which the present case appears to form an example. I say not very generally known, from having heard some of my brethren express surprise at the shortness of time (five hours) that intervened, between the seeing of Canning alive, and well, and his being found dead. It is true, that in the majority of cases of poisoning with arsenic, the time that intervenes between the administration of the dose, and death, is much longer than in the case before us; it generally extends from two to three days; but it has been satisfactorily shewn by Dr. Christison, that under certain circumstances, death takes place with great rapidity, and is then preceded by a very different train of symptoms. This chiefly occurs when the dose has been very large. The signs of inflammation are far from violent, or altogether wanting, and death takes place in five or six hours, a period too early for inflammation to be always properly developed. Vomiting may come on at the usual time in these cases, but it seldom continues; the most uniform and remarkable affection is extreme faintness, amounting at times to deliquium. Dr. Christison mentions eight cases of this variety of poisoning, to which I beg to refer the reader,*

* On Poisons, section Arsenic.

and I will conclude, by a short abstract of a very remarkable case of this description, which occurred in this city a few years ago, and I do so more particularly, because it appears to have escaped Dr. Christison's notice.

The case occurred on the 21st of May, 1828, and is recorded by Mr. Wright.* A young woman took an ounce of arsenic early in the morning. She suffered little or no pain or uneasiness for half an hour, when it is supposed she mentioned the circumstance herself. An apothecary's lad who then saw her, administered twenty grains of sulphate of zinc; this produced vomiting, which was encouraged by tickling the throat with a feather, and drenching her with warm water and sugar. This carried up a quantity of white powder, and was continued until the water came up clear, in which way two gallons of water were used. After the lapse of an hour the vomiting recurred, producing great exhaustion, which was followed by a cessation of all the symptoms. At this time Mr. Wright saw her, and found her perfectly free from all pain, even on pressure, rather drowsy and answering all questions, although not freely, yet quite collectedly. She was not thirsty, and there was no fetor of the breath. The pulse was so feeble and rapid, that it could not be counted, and the surface was bedewed with a cold clammy sweat. She said she felt exceedingly weak, and begged to be let alone, and allowed to sleep. In a very short time she expressed a wish to drink of something warming to her stomach, when a little peppermint water was given to her, after which she fell apparently into a sound sleep, from which she only awoke in death; about *four hours* having elapsed from the time the dose was swallowed. On dissection, scarcely any appearance of inflammation was found in the stomach, the only thing at all to be remarked of that nature, were two very small vascular spots, and a minute ecchymosis, but there were several large patches of viscid white paste, adhering to the mucous

* Lancet, vol. xvi. p. 612.

membrane, from which it could be easily scraped off, leaving no mark of injury beneath. There were about three pints of a reddish coloured fluid in the stomach, and in this, flakes of this white paste were seen floating. On testing the matter just alluded to, it was found to be arsenic. This is one of the most striking cases on record, and it exhibits in strong colours that variety of poisoning with arsenic, in which its local action is very trifling, and death is to be attributed entirely to its remote effects; the organs thus injured are the heart and brain, and the individual dies in a state of collapse and partial coma. In reference to these cases, Dr. Christison observes, "they are sufficient to correct an erroneous impression which some entertain, that arsenic, when it proves fatal, always produces violent and well marked symptoms." For further information upon this important and interesting subject, I must refer to the admirable work of the author just mentioned.

ART. XV.—Cases of certain Nervous Diseases, occurring principally in Females, with Observations. By SAMUEL CUSACK, A. B. M. B., Surgeon to the United Santry Dispensary, and Wellesley Female Institution, &c. &c.

[Read at a meeting of the Dublin Surgical Society, May 3d, 1833.]

THE necessity for correctness of diagnosis, in order to the successful treatment of disease, is so universally admitted, that any proof of its importance in the present advanced state of medical science, may be considered as altogether uncalled for.

While, however, the enlightened physician, by a judicious application of first principles, can in general discriminate between the most complicated affections, the inexperienced practitioner, as the following cases fully prove, may fall into errors calculated to entail on his patients unmanageable ailments, if not irrecoverable disease. My intention, however, on the present occasion, is not to dwell on diagnosis generally, but

to relate some cases illustrative of a peculiar form of nervous* disease, and to make a few observations on the affections which those cases exemplify.

CASE I.—Mrs. ———, six months pregnant, consulted me by letter, expressing her desire to be placed under my care during her confinement, which she looked forward to with great anxiety, having suffered such protracted illness on former occasions. She requested me to prescribe something for a pain in her right side, from which she was never free, describing it as being in [general dull, but occasionally most acute, and distressing, shooting through her shoulder.

She had on different occasions applied upwards of two dozen leeches to her side, used mercury externally, as well as internally, and was cupped and blistered without obtaining any relief. The want of success attendant on this plan of treatment, combined with the description of the symptoms given in the letter, induced me to consider the affection as one of a nervous character, and to treat it accordingly. With this view, I directed a tonic system of living, and prescribed three grains of the *pilula hydrargyri*, with a similar quantity of the compound galbanum pill, every third night, to be followed in the morning by a gentle aperient draught. Four years have since elapsed, and this lady has been twice confined under my care, and on each occasion has had as favourable a recovery as I ever witnessed. I have on different occasions examined the situation of her liver, and did not find it enlarged or in any way diseased.

She informs me, that the pain (which very rarely troubles her) always ceases on taking two, or at most three of the pills, which, to use her own words, act like magic. She does not take the draught, as it disagreed with her, and did not produce the intended effect on the bowels. On any return of the pain, she takes two of the pills at bed time, which have the effect of

* The term nervous is made use of in the following communication, in contradistinction to organic or inflammatory, and not in any definite sense.

gently freeing, and takes a third pill the following night, if the pain continues, but never finds it necessary to take a fourth. It is worth remarking, that a few days after each of her last two confinements, on feeling the pain, she asked leave to take the pills, and on doing so, the pain immediately ceased. Had she, instead of using these simple remedies, been subjected to as rigorous treatment, as on former occasions, might not a recurrence of her former sufferings have been the result?

Mrs. ——— requested me to prescribe for one of her domestics, whom she apprehended was becoming consumptive. She had nursed a child for her mistress about three years previously, and for some time after enjoyed very good health; latterly, however, she has lost all relish for food, become completely incapable of any exertion, and has fallen quite into a state of despondency. She never stirs outside the house, and from a pain in her right side, and constant palpitation, she looks upon her condition as completely hopeless. In reply to my inquiries as to her habits, and the remedies that had been used for her relief, she informed me, that tea constituted almost her entire sustenance, and that leeches had been applied to her side, which had the effect of weakening, but not relieving her.

Notwithstanding her delicate condition, having satisfied myself that no organic disease existed, I gave an encouraging prognosis, and I assured her, that her health would in all probability be completely restored by a strict adherence to my directions, which were, strict inhibition of tea or coffee, regular exercise of a gentle kind, attention to the temperature of the lower extremities, and the use of the following remedies;

R. Masæ Pilulæ Galbani Comp. gr. vii.

———— Hydrargyri, gr. iii.

Fiant pilulæ duæ, 3a. q. q. nocte sumend.

R. Infusi Quassiae, ℥xii.

Sulphatis Magnesiae, ℥iss.

Fiat haustus, 3a. q. q. mane sumendus.

After some weeks she was much improved, and at the expiration of a year, I had an opportunity of seeing her completely restored to health.

Mrs. M——, ætat. 29, wife to a police constable, has had pain for some time in situation of liver, with palpitation; this was completely relieved under the use of powders of carbonate of iron, and valerian, she had tried different other medicines, ineffectually.

Mrs. ———, ætat. 30, the mother of several children, has been subject for many years to a distressing pain of the right side, between false ribs and crista of ilium, she is also teased with frequent headach. When the pain was very severe, it completely disappeared on taking ten grains of carbonate of iron, a scruple of valerian, and five grains of effloresced soda, three times a day, and applying to the side the aromatic plaster of the Dublin College. This lady, from her mode of life, has occasional returns of the pain, and always derives the greatest benefit from taking a combination of compound galbanum and blue pill, and draughts of compound infusion of gentian, tincture of rhubarb, and sulphate of magnesia. She has used aloetic purgatives in other forms, and pills of aloes, assafoetida, and sulphate of iron, without any benefit.

Judy Cloony, dispensary patient, a person of intemperate habits, has suffered for the last two years intense pain to the right of the spine, between the false ribs and crista of the ilium. It is at present so acute, that she walks nearly doubled; she was bled and blistered when the pain commenced, without any diminution of pain. Finding that no relief was obtained by purgatives, and terebinthinate frictions, which I had often found serviceable in such cases, I directed a tea spoonful of acetate of iron, (the only preparation I had at the time), every second hour, desiring it to be discontinued if any unpleasant effect was produced. After using the quantity I gave her, (one ounce), she told me she felt more relief than she had ever before experienced: I then directed fifty drops of the muriated

tincture of iron, in a wine glassful of infusion of valerian, three times a day, under which treatment the pain completely disappeared, and has not since returned.

Mary H——, ætat. 14, for the relief of a severe headach of some months' duration, took an aperient pill daily, and, subsequently, had two issues opened on the top of her head, which were kept discharging for a considerable time by means of caustic applications. Much suffering and debility ensued, but no diminution of headach; latterly, the issues have been allowed to heal, the headach, however, still continues. She has never menstruated, is subject to chronic bronchitis, and frequently distressed with flatulence; her appetite is bad, her extremities cold, she leads a sedentary life, and makes use of tea twice a day. I treated this individual, by ordering the use of light animal food; and by inhibiting the tea, and insisting on exercise in the open air. Small doses of blue pill, with the compound galbanum pill, with an infusion of quassia with sulphate of magnesia, were directed to be taken every third day, ordering, in addition, the pediluvium; and in the progress of the case, I substituted a mixture composed of equal parts of the infusions valerian and quassia for the other medicines: under this treatment she has perfectly recovered. The catamenia did not appear for a considerable period after, notwithstanding the complete removal of all unfavourable symptoms.

The foregoing cases, selected from amongst many others of a similar nature, seem to be of considerable interest in a practical point of view. They exemplify diseases of a nervous character, in a great measure amenable to treatment of a mild nature, but aggravated, if not rendered altogether incurable by severe measures, which impair the peculiar constitutions in which those diseases are found to exist. On reference to the cases, it will appear, that three similar forms of disease exist, occupying different situations, and requiring with some a similar plan of treatment. It will also be seen, that those affections, when

their true character is not known, are liable to be mistaken for inflammations of some contiguous viscus, and treated as such.

The first of those affections which we shall consider are, what we shall term the hypochondriac pains, from their situation, one having its seat in the right, the other in the left hypochondriac region. The former of these pains is frequently considered indicative of inflammation, or disease of the liver, and treated by bleeding, leeching, mercury, purgatives, &c.* The left hypochondriac pain the author has seen treated as inflammation of the pleura. The situation of these pains is not uniform. In some instances they are felt inferior to the mammæ on the convex part of the ribs, sometimes they are situated immediately below the false ribs, or midway between them and the crista of the ilium, occasionally to the corresponding shoulder, hip, and thigh, or shoot across from the spine to sternum, as if traversing the diaphragm: not unfrequently the right and left hypochondriac pains are found to alternate with each other. These pains are in general of an obtuse character, but liable to distressing exacerbations, there is frequently great uneasiness produced by pressure, or even touching the integuments, this seems, however, to be the result of mere morbid sensibility of the part.

The third form of the disease is the pain of the head, or cephalic pain, which, like the hypochondriac pains, is not uniform as to situation or extent. It is sometimes confined to a small spot near the orbit or vertex, and sometimes spreads over the entire head. From the description given to the author of this affection, in many instances it did not appear to him to be so much a pain, but rather a kind of morbid feeling or sensibility carried to a high degree. In the cases which the author witnessed, there was not any unusual state of vision attendant

* A case illustrative of this affection is alluded to by Dr. W. Stokes, in his able paper on Inflammation of the Liver, in the *Cyclopædia of Medicine*, and Andral's opinion quoted on the subject.

on the pain. Many persons labouring under this affection, experienced a sensation as if the hair was dragging out by the roots, and not unfrequently complained of intense cold of the head, and a feeling as if cold water was pouring on it. In such cases it is not unusual to heap flannel caps on the head, a proceeding which aggravates the disease. With respect to the precise seat of those affections, or as to whether any appreciable change is produced in the internal organization of the part engaged, an opinion can only be formed from conjecture, as pathology does not throw any light on the subject. It seems probable, however, from the fact of all those affections being found to alternate, and even sometimes to co-exist in the same individual, as well as from the similarity of the hypochondriac pains of either side, that their seat is in the more superficial parts of the body, and not in the contiguous viscera.

The diagnosis of those affections is not to be formed without a familiar acquaintance with the symptoms of the diseases which they simulate; independently, however, of the aid afforded us by a comparison of symptoms, some collateral circumstances may give us much assistance in determining our diagnosis; for instance, the comparative frequency of those affections, and the diseases which they simulate. The affections under consideration are of frequent occurrence, the diseases which they simulate comparatively rare. Secondly, should the diseases have been of long continuance; the effect of former treatment. Thus if those affections have been aggravated, or not alleviated, by an antiphlogistic plan of treatment, it will tend to shew that their character is not inflammatory. We must, however, bear in mind, that treatment, which in reality is injudicious, may occasionally give temporary relief; for instance, those pains will occasionally be lessened by the application of leeches, a circumstance to be accounted for by the inexplicable fact, that nervous pains can, in many instances, be relieved by the development of a new action. Leeching, however, though in some instances capable of producing this effect, is calculated, if carried too far,

to induce that peculiar state of the constitution, favourable to the development of those diseases. Thirdly, the sex of the patient. Females are far more liable to those affections than males, though the latter sex is by no means exempt. Fourthly, the constitution of the person affected. It is in persons of a nervous, hypochondriac, and dyspeptic constitution that those ailments are found most frequently to exist. Lastly, much assistance in forming a diagnosis is to be derived from the habits of the patient and mode of life ; should a female, who is constantly confined to the house at her needle, as milliners in large cities frequently are, who drink a large quantity of tea, and have no relish for other food, complain of pain in the situations we have been describing, we may, without further hesitation, rank the disease in the class under consideration. Two circumstances, however, should never be lost sight of in the diagnosis of those affections, first, that a person affected with those nervous diseases is by no means exempt from the existence of disease of a more dangerous description : an individual, such as we have been describing, may, in the first instance, labour under any of those affections, especially the hypochondriac pains, and may, if injudiciously treated, particularly if she be of a scrofulous diathesis, ultimately fall a victim to phthisis : secondly, any of those pains (though to the person affected apparently uniform) may arise from different causes, be totally dissimilar in their nature, and necessarily require quite an opposite system of treatment. Thus the hypochondriac pains may at one time be neuralgic, and at another time be indicative of inflammation of the stomach or duodenum, or of the pleura : in like manner we find that headach may in the same person be at one time the result of plethora or inflammation, and subsequently, without any intermission, assume the nervous character we have been describing.* This appears to us to be a most

* We conceive, however, that acute pain of the head, or of the hypochondriac regions, is less likely to be inflammatory, when pain in those situations has previously existed in its nervous form.

important point of consideration in the treatment of cephalic affections, that a headach may, in the first instance, be inflammatory, and without any interval, during which the patient is free from pain, run into one of quite an opposite kind. We recently attended a lady who laboured under intense headach, accompanied by great determination of blood to the head; it was lessened by the application of a large number of leeches. The relief afforded was not of long continuance, and the lady, judging by her own feelings, requested that the leeches might be re-applied. Being fully persuaded, however, that the character of the headach was altered, we prescribed musk, valerian, and Dover's powders in large quantities, and the result fully answered our expectations.

It is not our intention to enter at any length into the consideration of the treatment of those affections, as the cases which have been related, in a great measure exemplify what we have to offer on that subject. We believe a removal of the exciting causes is, in many instances, sufficient for the cure of the disease, and in any instance of disease, without such removal, all other treatment must be ineffectual. Thus if we find that sedentary habits induce those diseases, exercise is to be enjoined. The nature and degree of exercise must, of course, be suited to the individual case, and will evidently defeat the object in view, if carried so far as to produce fatigue; sometimes walking, at other times horse or carriage exercise will be found most beneficial. When we are prescribing for young females in the higher classes of society, we should bear in recollection, that in many instances so much time is devoted to study and the acquirement of what are termed accomplishments, that the health is often sacrificed thereto. The headach we have described is frequently aggravated, if not entirely caused by the quantity of hair worn by some females; when such is the case, the hair should be cut close, and a light cap worn. The feet of individuals, subject to the pains we have been describing, are, in general, uncomfortably cold; every means should be employed to

preserve a natural temperature, and immersion in warm water every second night, or oftener, with subsequent friction, by means of a towel wrung out of salt water and dried, or a flesh brush, will be found most serviceable. In many instances those affections are entirely attributable to the use of tea: this is particularly the case amongst females in the lower ranks of life, who render this beverage less wholesome as well as palatable by boiling instead of infusing it, and frequently live almost entirely on this fluid. Where tea is found to produce this effect, its use should be interdicted, and cocoa, whey, or some other drink substituted. Coffee will frequently be found to produce the same unpleasant effects as tea; where such is the case, a very weak infusion of the burned roots of the wild succory or chicory, with a large proportion of boiled milk or cream, will be found an agreeable and wholesome substitute.

Attention to diet must evidently be of paramount importance. The kind of food, however, that is found to agree with the class of patients we have to treat, is so well known that it is unnecessary to dwell thereon; we shall, therefore, be content with giving the caution of not using solid or stimulating food in cases where the hypochondriac pains are the product of inflammation, as much aggravation of inflammation and pain must be the result.* The medicines serviceable in the diseases under consideration are such as improve the state of the digestive system and the general health; subdue nervous irritability, and relieve pain. And in many instances, those remedies will be found more useful when employed conjointly. Without wishing to attach undue importance to any one remedy, we would

* Females of a weakly, nervous, constitution, often find a degree of temporary benefit from the use of wine. Its effect, however, is but temporary, and after the excitement produced by it is over, an aggravation of the nervous symptoms is almost always the result. We should carefully avoid allowing such patients to acquire the habit of using unnecessarily æther, ammonia, and such like stimulants.

mention what in our practice has been found most frequently useful, namely, small doses of the pilulæ hydrargyri alone, or combined with one or two parts of the compound galbanum pill, every second or third night, followed in the morning by draughts of infusion of quassia, holding a small quantity of sulphate of magnesia in solution : we have found this combination so frequently beneficial, that we would have recourse to it in preference to any other remedy, where there was no circumstance to contra-indicate its use. The infusion of quassia, combined with the tinctures of rhubarb, gentian, or columbo, have in many instances proved serviceable. In some rare cases, where those remedies failed in giving relief, we have seen benefit arise from the employment of active purgatives, as jalap and calomel, the compound colocynth pill, with calomel, &c. In some few instances, where the pain of the left side was so acute as to resemble inflammation, and the patient had solicited us to use the lancet, we have completely relieved the pain by active purgatives.

We have frequently found the chalybeate preparations of the greatest benefit; the preparations which we most generally employed were the carbonate of iron, in the proportion of ten grains, with a scruple of valerian, and ten grains of dried soda in syrup of ginger, three times a day. We have also used the muriated tincture of iron, and acetate of iron, in draughts of infusion of valerian, or infusion of quassia combined with tincture of colombo. As to local treatment, we have not found any benefit from the application of leeches, except in the hypochondriac pains, when resulting from inflammation, in which case the application of leeches, or cupping, should invariably be had recourse to. We are unable to speak from personal experience of the application of tartar emetic ointment or blisters. In the cases where we witnessed their employment, they seemed only to increase the sufferings of the patient. We have, however, seen the greatest benefit apparently arise in the hypochondriac affections from the application of the aromatic plaster, and have

been informed by others of its beneficial results.* The greatest relief has almost invariably been derived by the persons under our care affected with the cephalic pain from the employment of the popular remedy, valerian. This we have exhibited in the form of infusion alone, or combined with the simple or volatile tincture and syrup of ginger. In many cases of this pain, with great disturbance of the nervous system, we have found half a drachm of the volatile tincture of valerian produce the most refreshing sleep.

In conclusion, we trust, from the cases related, that it will appear that those troublesome affections which form the subject of the present communication, are in all instances capable of considerable alleviation, and in many of perfect cure. While in most instances a glance of the experienced eye is sufficient to recognize those diseases, we must confess that there have been instances where we have been in doubt to what class of disease those cases were referrible. Such instances, however, were but rare, and we never have had reason to regret adhering to the following rule of practice. If in the event of the diseases being inflammatory, procrastination would possibly have been of serious consequence, we have run the risk of erring on the safe side, and treated the diseases as if they were inflammatory; and where there was no circumstance to contraindicate it, we have, in conjunction with antiphlogistic measures, employed such means as would be calculated to relieve the disease if of a nervous character. Where, however, there was a strong probability, but not a certainty, of the disease being nervous, and no danger to be dreaded from delay, we treated it as nervous, watching most closely the effect of our re-

* We are not aware whether a pain of the left side alluded to in the article *Hysteria*, in the *Cyclopædia of Practical Medicine*, is the same as that we have been considering. If so, we have been more fortunate in the cases we have had to deal with than the author of that paper, as he has found the pain most unmanageable. The reverse has been the result with us.

medies, to be ready, if necessary, to resort to measures of quite an opposite character.

ART. XVI.—*Pathological Researches.—Inflammatory Affections of the Brain, and its investing Membranes.*—By W. I. MORGAN, A. M. M. D., Vice-President, and One of the Censors of the King and Queen's College of Physicians. Physician to the Whitworth Hospital, &c.

THE practice of the physician frequently presents to him cases fraught with interest and instruction, but which, in general, fade from his memory, and become lost both to him and the profession, unless he may have some favourite theory to support, or point of practice to illustrate; and then, the details appear under circumstances at least open to suspicion, and must come shorn of half their value, when those of successful issue alone are introduced. It has long been my opinion, that a simple, faithful portraiture of diseases as they appeared at the bed-side of the patient, as closely copied from nature as fallible judgment, and the defective medium by which we must convey them, can depict, together with a record of the treatment and its result, without, however, any intentionally direct reference to particular views either of theory or practice, would contribute more to the improvement and stability of medicine, than all the fine-spun theories, which have so long served to gratify the “constructiveness” of ingenious inventors, and disappoint the hopes of the sober practitioner. From cases in which our endeavours have been crowned with success, much may undoubtedly be learned; yet there are others of still higher importance to the profession; I mean those in which, after all our science, and all our skill have been put forth in vain, the cause of failure has been detected by an examination of the diseased parts, after death. How few, comparatively, of such cases are to be found on record! Much, however, as we may regret the circum-

stance, we cannot be surprised at the fact. The publication of successful cases has much in itself which is grateful, and nothing repulsive to our vanity, self-esteem, and love of reputation: on the other hand, there is something humiliating in recording our own defeat, subjecting our opinions to public scrutiny, and bringing our professional judgment to the palpable test of post mortem examination. Would that this were all! There is something still more galling to be feared. The love for Science is not yet, among all the members of our profession, so purely platonic, as to induce them to court her for her own sake alone. An inviting opportunity of raising our fame on the ruin of that of our neighbour; of exalting our self-complacency at the expense of his feelings and reputation; of blasting all that is dear to him with a sarcastic sneer—indicative of the much we could say, if we gave utterance to all our weighty animadversions—these, and much more, all equally at variance with professions of fidelity to her service, or attachment to her culture, are not considered incompatible by those who profess to be her votaries. What generous mind would subject itself to such a return for faithful services performed? Who would voluntarily submit to be dragged forth as a culprit to the bar of public opinion, to hear his own candour converted into subject-matter of accusation; to be twitted on account of supposed ignorance, and have his own very concessions cast in his teeth? The man who has the honest boldness to give to the profession a faithful history of unsuccessful cases, must be impelled by some good motive, and should be protected by every friend to science, to honour, and to humanity, not merely from the captious objector, but from those also, who, however well-meaning, judge and pass sentence without reflecting, that the candour of the writer has placed them in circumstances widely differing from those which he himself had occupied, as the practitioner. No doubt, no difficulty, no obscurity clouds the reader's view. He can see the event from the beginning; glance in one moment, from the first premonitory symptom, to the morbid appearances found on dissection, and then, return

again with a mind instructed in the nature and the result of the disease, qualified to scrutinize every progressive step in the treatment, and to form a very close estimate of the correctness of the views by which those measures were dictated—advantages, it should be remembered, which the practitioner never enjoyed.

Having accustomed myself, during some years, to premise my daily prescriptions with a short but accurate sketch of the most prominent symptoms of my patients, as I stood by their beds in the hospital, and occasionally to note those also in private practice, after retiring from the sick room, I determined, at some time, to give these sketches to the profession as public property; yet that time might never have arrived, but for the recent observations on the cases published by an eminent surgeon of this city. I could not avoid reflecting, that few seem to be aware how insidious and treacherous inflammatory affections of the brain and its investing membranes may be. The very mention of such affections is associated in our minds with the presence of great constitutional disturbance, high mental excitement, or its very opposite, total insensibility—in fact, with phenomena, so obvious and so permanent, that they could not fail to arrest attention and excite apprehension: yet these phenomena may be absent, or so indistinct and so fugitive as to be of no practical benefit. I do not mean to say that we shall not find symptoms to guide us in our practice, and to premonish us of the result; but this I assert, that cases occur in which the symptoms are by no means adequately indicative of the urgency of the disease, and that, without a caution and circumspection, for which we are not always prepared, the most experienced may occasionally find that he had been indulging a false security.

Possessing the particulars of a case, interesting in itself, and in many points similar to the first of those given by the talented individual to whom I have alluded, it appeared to me that some further illustration might be afforded by its publication,

and that I ought not longer to postpone my determination. My object, then, in these preliminary observations, is to explain my motives and intentions, and to bespeak from my readers some little patience and forbearance.

On the accumulated knowledge of physiological and pathological facts, the improvement of medicine, as a science, must mainly depend : the very error, under the oppressive effects of which our profession has been so long struggling, was too hasty generalization. Into the truth of facts, therefore, or whatever comes to us professing to be such, a scrutiny can never be too severely instituted, but in the very exercise of the most critical scepticism, the most rigorous investigation, we should still bear in mind, that between the "*suaviter in modo*," and the "*fortiter in re*," there is nothing essentially inconsistent.

On perusing the case which I shall now submit, as the first of my promised series, and which I shall copy as it was recorded nearly four years since, I do not feel that I have much on account of which to accuse myself ; yet, though the instability of the symptoms kept me in painful suspense, as to the progress and event of the disease, I freely confess that I did not anticipate the sudden demise of my patient, nor expect to find one hemisphere of the brain nearly bathed in purulent matter, and a portion of the dura mater detached from the temporal bone. But the case shall speak for itself.

CASE I.—Mr. ——— æt. 24, was admitted as a fever patient to the private ward in the Whitworth Hospital, Royal Canal, on the 24th of August, 1830.

Report.—Three weeks since caught cold, followed by extremely severe pain in left ear, pulsating and extending to left frontal region. Has been increasingly worse during this period till to-day, when he feels himself better. No discharge from, nor apparent inflammation of, external ear : has taken much purgative medicine. Was under the care of Mr ———, apothecary. Had been blistered behind ear. Pulse 104, soft ; tongue white in centre ; skin warm. Has taken two pills this morning.

R. Carb. Sodæ ℥ss.
 Sacch. Albi ℥iii.
 Solutionis Antimonii ℥ss.
 Tincturæ Opii, gutt. xx.
 Aquæ Fontanæ, ℥viiss. ℥.

St. coch. duo cum uno succi limonis inter effm. 2dâ quâque horâ.

N. B. Should the pain of ear return in the evening, six leeches should be applied in its neighbourhood, followed by a bread and milk poultice over the ear, when the leeches fall off.

25th. Leeches and poultice applied. Very little relieved by means adopted yesterday. Obligated to have an anodyne* in the night. Bowels not free. Tongue loaded at root. Pulse 80, bounding. Profuse perspiration during night. Pain extends from left ear over left frontal region ; increased on pressure. Inflammation of conjunctiva of left eye.

Mittantur Sanguinis ex Arteria Temporalis unciz decem.

Bol. Purg. (Subm. Hydrarg. gr. v. Pulv. Jalapæ, gr. xv.) q. p.

Mist. Purg. vesperi, si opus sit.

26th. Slept well during night. Much relieved till half past twelve to-day, when he was suddenly seized with excruciating pain of ear, screeching and delirium. Is still delirious. Words incoherent, and like a foreign language. Pulse 96, full : skin hot : trembles and complains of chilliness : tongue moist, slightly white : bowels well freed : pain not so great.

Mittantur sanguinis e Brachio unciz ——.†

* For this practice, whether right or wrong, I am not accountable, as it was adopted by the resident apothecary, on his own judgment and responsibility. I am aware that some are averse to the use of any portion of opium in head affections ; but our judgments on this, as perhaps on other points of practice, may be prejudiced by what we have witnessed in a few instances ; some patients being as remarkably susceptible of the bad effects of opium, without any of the good, as others are of being benefited by all the good, without experiencing any of the bad.

† Thus it is written in my prescription book ; for I had given a verbal order to measure the blood drawn, not by quantity, but by effects, and to allow it to flow as copiously as the strength of the patient would permit. I regret, merely as a matter of curiosity, that I have no record of the amount taken on this occasion.

R. Submuriatis Hydrargyri grana quindecim.
Pulveris Antimonialis grana sex.
Opii granum.

Ft. pilulæ sex. Sumat unam horâ omni alterâ. Foveantur pedes vesperi.

27th. Very much relieved. Says he suffers no pain. Quite composed. Face flushed. Pulse 80, soft : skin moist : tongue white : no stool.

R. Sulphatis Magnesiae ʒxii.
Tart. Antimon. grani dimidium.
Aquæ Font. ʒviiss.
Tincturæ Jalapæ, ʒss. m.

Ft. mistura cujus sumat coch. duo, horâ omni, donec alvus soluta sit.

Enema Purg. vesperi.

28th. Was very well till half past ten o'clock last night, when pain suddenly seized him in the ear again ; since which has been affected as on 26th. Complains much of pain in small of back. Head now hot : pulse 80, full and hard : tongue white and loaded : bowels smartly freed during night : speech incoherent : pain of ear not urgent now.

V. S. ad ʒxii.

App. Emp. Canth. Nuchæ.

R. Massæ Pilulæ Hydrargyri, gr. xv.
Tartari Antimonii granum.
Opii grana duo.

Ft. Pilulæ sex. Sumat unam horâ omni alterâ.

N. B. If pain of head should continue after venesection, let twelve leeches be applied to temples.

29th. Pain allayed. Says it is often absent for six or eight hours. Pulse 96, not very sharp. Tongue loaded and white. Bowels well freed. Tenesmus. Much general tremor. Head very hot. No external marks of inflammation. Mind quite sensible.

Applicentur Hirudines duodecim temporibus.

R. Hydrargyri cum Creta grana decem.

Divide in partes sex. Sumat unam 3tiâ quâque horâ.

Haust. Olei Ricini primo manè.

30th August.—Died at six o'clock this morning.

He had been rather easier during the night, and until this morning, when he complained of a sense of suffocation; was raised up at his own request; spoke intelligibly; said he felt faintish; was laid down, and soon expired.

Autopsia, thirty Hours after Death.—Present, Doctor —, the cousin of the deceased; Robert Adams, Esq., A. M., the Surgeon to the hospital; and Dr. Robinson, one of the Physicians to the Fever Hospital, Cork-street, a casual visitor. The dura mater, which presented nothing remarkable, having been removed, the brain was found firm, and, when cut into, its medullary substance was a little more vascular than natural. That portion of the brain which corresponded to the region supported by the temporal bone, was covered by a layer of yellowish-green puriform lymph. This lymph was visible through the transparent arachnoid membrane which covered it, and appeared to be partially spread over the convolutions of the brain, sinking into their sulci,—in short, it was infiltrated into the spaces between the arachnoid and pia mater of the left temporal region of the brain.

The inflammatory irritation which had produced this imperfectly formed lymph, had not confined itself to the exterior membranes, for the arachnoid-lining of the ventricles, as well as the spinal arachnoid, had produced a quantity of clear serum, which flowed out when the ventricles and spinal canal were laid open.

As the paroxysms of pain had been referred to the ear, during the few days he was under my observation, we next directed our attention to this organ. In the external ear and meatus, nothing unusual could be discovered; but when the

left hemisphere and middle lobe of the brain had been fully raised up from the petrous portion of the temporal bone, a thick bed of yellow lymph was found, intervening between the anterior surface of this part of the cranium and its proper covering, the dura mater. This latter was separated here for the space of half an inch square, from that part of the temporal bone which overhangs the roof of the labyrinth of the internal ear. This part of the petrous bone was somewhat softened, as it admitted the knife, with some little pressure, to sink into it.

The state of the petrous bone, already described, and the separation of its periostial, or dura-matral, covering, by a thick bed of yellow lymph; secondly, the puriform effusion, or infiltration of lymph, underneath the arachnoid of the left hemisphere; thirdly, the serous effusion into the interior of the ventricles and spinal canal, with some increase of the vascularity of the brain, were the morbid phenomena detected on examination, whence originated these variable but insidious symptoms, and which finally terminated in the death of this gentleman.

I shall now subjoin a few observations on the more remarkable features of this case. This disease must have been most insidious from the very commencement: for, first, the apothecary, who had been attending the patient for three weeks previously to his admission, never adopted more active measures than cathartics and a blister behind the ear, and at the end of that time sent him to me as a fever patient. Secondly, his cousin, who is a physician in good practice in a branch of our profession, in this city, and who visited him, gave him in charge to me without expressing his dissent from the opinion delivered by the apothecary. Thirdly, when I saw him, no pain, nor any other symptom, indicative of the real seat of the complaint, was present, and the first day's record is occupied chiefly with the detail of past, rather than present symptoms. Fourthly, during an attendance of five days I never saw him in a paroxysm of pain: once only, viz. on the 26th, I witnessed the effects produced

by it, and it was in reference to this fact,—the absence of pain, that, but a few hours before his death, I obtained the remarkable answer recorded under that date as to his freedom from pain, and the duration of its absence. Fifthly, whatever he may have suffered during the presence of pain, he had perfect intermissions, and at each return it observed a certain period of aggression: both of which symptoms were formerly assumed as the distinctive marks by which the presence of inflammation might be negatived. Sixthly, as such extensive morbid changes could not have taken place either at or after his decease, they must have commenced and progressively advanced, perhaps from an early period of the attack: yet we have already seen that he had intervals of perfect immunity from every urgent symptom; lastly, at the very moment of time when those changes had arrived at their full amount, and in so much that they ceased to be longer compatible with life, his mind was in perfect enjoyment of intelligence, and neither delirium, nor coma, nor convulsions, announced the affection of the brain or of its membranes. Perhaps it is my duty to add, that subsequently to the decease of my patient, I learned, on good authority, that this fatal attack was attributed to imprudent exposure to cold by bathing in the sea, too soon after the employment of mercury in the removal of syphilis.

Within the last two months I have witnessed another case, which, though not so interesting in detail, is equally illustrative of the fact, that inflammatory action may commence, proceed, and terminate in the formation of purulent matter, in the membranes of the brain, without exciting any of those symptoms by which we could pronounce that such a formidable process had been established there. I shall condense the report as much as possible.

CASE II.—Sarah Johnstone, ætat. above 60, had been under medical treatment on account of cough and difficult respiration, the effects of catarrh originating in exposure to wet, six days previously to her admission into the hospital.

When she came under my care, 16th December, 1833, her most urgent symptoms were pain in epigastrium and right hypochondrium, and obstinately constipated bowels, but which yielded to the application of eight leeches to right hypochondrium, and a blister to epigastrium, accompanied by the steady exhibition of mild purgatives, which succeeded on the third day in bringing off copious hard, dark stools. Pulse 100-112. Tongue loaded. Her strength appeared to be so broken, that from the second day, the citrate of ammonia, with camphor mixture, were directed for her. On the fifth day from her admission, an alarming prostration of strength suddenly supervened. No active nor debilitating measures had been employed, by which to account for this sudden collapse. Yet her pulse was indistinct, and stools passed involuntarily. Re-action commenced on the seventh day. Her pulse became distinct, at seventy-five. Tongue clean : heat of surface returned : symptoms remaining stationary till the eleventh day, when the eyelids became tumid : tongue covered with dark sordes : pulse seventy, and feeble. At this period erysipelatous inflammation suddenly appeared round the palpebræ of the left eye. The globe of the eye and all the surrounding parts were protruded forward, giving to the face a very frightful aspect. These local symptoms increased. The eye was directed inward. Vision indistinct ; finally lost. Incisions were made into upper and lower eyelid, by Mr. Adams, but no purulent matter was discharged. Finally, both eyes were protruded, with total loss of vision. Erysipelatous inflammation surrounded the palpebræ of the right also, but the disease had the priority in the left, by three days. On the morning of the fourteenth day from her admission, and the third from the appearance of the inflammation of the parts surrounding the eye, she expired, without any symptom indicative of inflammatory action having extended beyond the palpebræ, and the parts contained within the orbits. Small portions of wine ; citrate of ammonia ; blue pill and carbonate of ammonia ; a blister to nape of neck ; occasional draughts of pulv. rhei ; infusion

of senna, and camphor mixture; and saline draughts; constituted the chief treatment from the period of collapse—the application of the blister being subsequent to the appearance of the inflammation.

Autopsia, thirty-four Hours after Death.—Present Mr. M'Dowel, one of the Surgeons of the Richmond Hospital; Mr. Adams, the Surgeon, and Mr. A. Robinson, the resident pupil of the hospital. The body was much emaciated. The globe of the left eye advanced beyond its orbit, and appeared between the palpebræ, although they were much swollen. An incision made between the roof of the orbit, and the attachment of the left superior palpebra, gave exit to pus mixed with a little blood. The dura-mater and other membranes, on a superficial examination, appeared natural, as did the brain itself, but when its basis was cut into, it was found softened and infiltrated with serum, particularly in the left anterior lobe where it lay over the orbit. At this part, also, beneath the arachnoid, and between it and the pia mater, there was a deposit of a greenish purulent lymph which seemed to follow the track of the optic nerve into the brain. This lymph was spread extensively over the inferior and lateral surface of the whole of the left anterior lobe. Some watery effusion was found in the ventricles; the presence of which might have been expected where so much irritation had existed on the surface of the brain. The thickening of the optic nerve, its enlargement at its entrance from the brain into the orbit, and the remarkable protrusion of the eye from its socket, all directed our attention to that cavity, and called for a careful examination of the nature and state of its contents. That this latter phenomenon was not caused by any free fluid which was within reach of the scalpel, and could be evacuated by an incision, was already proved by experiment on the same subject, both while living and dead. The triangular bony roof of the orbit, detached at each side from the rest of the frontal bone, was carefully raised: the whole of the periosteum covering the bony arch

of the orbit, was found but loosely adherent, and a very careful examination evinced, that, at one spot, true pus existed between the roof of the orbit and its investing periosteum. The optic and other nerves, the muscles, the cellular and adipose membranes; in short, the whole contents of the cavity, the globe itself excepted, exhibited the effects of a severe and rapid attack of inflammation. The optic nerve was thickened, indurated, and covered with a tenacious layer of yellow lymph, while the surrounding yellowish-green, adipose, cellular membrane seemed to be in nearly a sloughy condition. It was the mixture of pus in the interior of the orbit, and the tumefaction of all the structures surrounding the optic nerve, including the lacrymal gland itself, an almost indistinct mass, which had caused that most remarkable feature in this case, namely, the partial protrusion of the globe of the eye. The extension of the inflammation along the tractus opticus into the interior of the brain, the effusion of serum into the ventricles, and especially the purulent infiltration beneath the arachnoid of the basis of the brain, account for the fatal catastrophe, and the preceding symptoms; for the other viscera were sound, if we except enlargement of the liver, and strong adhesions of the pleuræ connecting the right lung to the sides of the thorax; neither of which could have had influence in hastening the fatal event.

Here, then, is another well-marked instance of inflammation of the membranes of the brain, severe enough to deprive the patient of life, yet without coma, delirium, pain in the head, or in fact any obvious symptom by which the extension of the inflammatory action from the parts within the orbit, to those within the cranium, could be ascertained. Many reflections here suggest themselves to the inquiring mind, but in accordance with the principles of my design, I give the facts, faithful and unadorned, leaving them to speak their own language, and convey their own instruction.

ART. XVII.—*Cases of Uterine Hemorrhage*. By FLEETWOOD CHURCHILL, M. D., Licentiate of the King and Queen's College of Physicians in Ireland, Physician to the Wellesley Lying-in-Dispensary, and Lecturer on Midwifery, &c. &c. in the Medico-Chirurgical School, Digges-street, Dublin.

[Read before the Surgical Society of Ireland, Saturday, January 25, 1834.]

ON a subject so important as hemorrhage from the uterus, where the most prompt and suitable treatment is required, and where but too often our best devised plans fail, no apology I trust can be necessary for bringing before the society a detail of four cases which have occurred to me in the practice of the Wellesley Dispensary. I do not assume to myself the credit of any thing new in practice or profound in theory. It will be seen that in the two first of the cases I merely state the results of greater promptitude of action, and in the latter exhibit, if not an unknown cause of flooding, yet one that is not noticed by authors as producing its peculiar effects at that particular period. And although in the cases to be recited, the remedies employed were inefficient,—the patient who recovered appearing to owe little to the means used,—still as throwing light upon one variety of uterine hemorrhage, I think they are of considerable interest and importance.

CASE I.—Mrs. Kennedy, æt. 27, of fair complexion, red hair, and delicate health, having had two or three miscarriages, was attacked with slight hemorrhage in the beginning of May, 1832, which ceased spontaneously. She sent to the dispensary at ten P. M. on Friday, May 25, 1832, on account of a severe flooding, which came on after a fit of crying without apparent cause. One of the senior pupils visited her, and administered the usual remedies, but finding them ineffectual, I was sent for. I found her blanched, faint, and complaining of excessive exhaustion. Pulse very small, wiry, and uncertain. There were no labour pains, but slight stings in

the belly were followed by the expulsion of clots from the vagina ; in this way she had lost a considerable quantity of blood. She stated herself to be within a month of her full time.

On examination, the vagina was found very hot, tender, and irritable, and the os uteri so high, that it was with great difficulty I could reach it without being able to ascertain the presentation. No hemorrhage followed the examination. As the flooding had greatly diminished, I directed her to be kept quiet, to have cold applied constantly to the vulva, and to take a large dose of laudanum.

At six o'clock on Saturday morning, she was visited by my colleague and myself. Some more clots had been passed, but not in great quantity. Pulse weak and quick. The os uteri was quite beyond reach, unless by introducing the hand. This the patient would not permit ; it was indeed not without difficulty we obtained an examination at all. As but little flooding was going on, we determined to wait awhile, that she might rally. She was ordered *infus. rosæ c. acid. sulph. dilut.* Continued cold to vulva. A plug of soft linen was introduced. My friend, Dr. Darley, saw her, with my colleague, Dr. Maunsell, and myself, about two P. M. She appeared better, and refreshed by sleep, with scarcely any draining. Dr. Darley was just able to reach the os uteri without ascertaining any thing as to the presentation. After careful consideration of her state, and the advantages of immediate operation, and of delay, it was agreed upon to allow her to rest awhile, unless the hemorrhage returned.

At ten P. M., she appeared rallying very quietly and steadily, and it was determined, that if no hemorrhage recurred during the night the operation of turning should be performed early the following (Sunday) morning. At half past seven, Sunday morning, I was summoned, as the flooding had returned. Dr. Maunsell accompanied me, and we found her blanched, covered with cold, clammy perspiration, and without pulse. She refused

to take any thing, or to allow an examination. She became very restless, throwing herself about in the bed, exclaiming, "that she did not know what was the matter with her;" then stretching herself out, she breathed shorter and at longer intervals, and in a few minutes died. There was no draining during our visit, and it was thought she might have lost a pint this last attack. Permission to examine the uterus was obtained, and the placenta was found implanted over the os uteri, from which it had been detached to the extent of about half an inch posteriorly.

On a review of this case, two reflections will probably occur to the minds of most; first, that an accurate examination as to the presentation ought, "*coute qui coute*," to have been made; and, secondly, that the operation should have been attempted at an early period. However evident and easy this may appear now, many difficulties opposed themselves at the time. The irritability of the vagina, the obstinate refusal of the patient to permit more than the slightest examination, and the danger, by disturbing the clots, of reproducing hemorrhage, which in her exhausted state, might prove fatal; all were arrayed against our wish for a more perfect investigation. Her exhaustion, and the disposition to rally quietly and steadily, with the little draining, made us wish to defer the operation until it could be performed with the greatest advantage to the patient.

Having said so much as to the motives which actuated us, I may state fairly and candidly my own conviction, from the experience of the case recited, that an early operation would afford a greater chance of recovery, even though a rally had not taken place; and my own determination, should I be called to other cases of this kind, to deliver the patient as soon as possible. In saying this I would not be understood as asserting that instant delivery is either advisable or possible in *every* case. There may be some reasons, from the state of the patient, which may postpone or prohibit the operation, or the patient, as in the above case, may refuse to submit to it. But in the majority of

cases, where the os uteri is dilatable, the danger, which is doubled by delay, is not increased by operating immediately without waiting for reaction. The following case will illustrate my meaning.

CASE II.—Mrs. Brennan, ætat. 30, mother of two children at full term, and after natural labour. Sent up to the Wellesley Dispensary, for assistance, at midnight, May 10, 1833. A pupil visited her, and found her much exhausted from severe flooding. She stated, that for the last week she had lost a pint of blood daily, and considerably more previously to sending to the dispensary.

On examination, he found what he thought was a placental presentation, and I was summoned. Before my arrival some slight uterine contractions occurred, and I found the os uteri dilated to the size of half a crown, very high in the pelvis; with a slight membranous pouch protruding. I ruptured this, and a great quantity of liquor amnii escaped. The head of the child presented. Labor pains came on regularly, and the flooding ceased, and all seemed going on well. In about half an hour, however, a change took place, the pains became irregular and more feeble, a dribbling of blood was observed, and she complained of faintness. The vagina was found full of clots, and I could no longer reach the presenting part. The pulse became extremely feeble, often scarcely perceptible, and irregular. I exhibited some stimulants, and sent to request the assistance of Dr. Darley, to whose kindness on this and other occasions I stand deeply indebted. On his arrival, it was determined, notwithstanding the unfavourable state of the patient, to proceed instantly to deliver. At this time the pulse was imperceptible at the wrist, the heart's action feeble and fluttering, and the surface cold. I had no difficulty in introducing my hand, which came in contact with the right side of the thorax of the infant. The extraction was performed with little pain, and followed by no hemorrhage. The placenta was expelled in half an hour, and the uterus contracted firmly. The

bag of the membranes was found entire, except a perforation at the edge of the placenta through which the child passed. The child of course was dead. At the termination, the pulse was just faintly perceptible; the patient, however, said she felt somewhat better. She was left under the care of an intelligent pupil for the night. Fifteen drops of laudanum in cinnamon water were ordered as a draught, to be given every half hour, until some narcotic effect should be produced. Wine was ordered, but she refused to take it. After a few doses of the laudanum she became quieter, and towards morning obtained some light sleep. Her skin became warmer, and no hemorrhage occurred. On visiting her early next morning, I found her much better than we had anticipated. She felt very weak. Her pulse were quick and feeble, yet distinct. Skin warm. No after pains. Lochia scanty. She had taken about 180 drops of laudanum, and appeared a good deal under its influence. It was now discontinued, and from this moment, without aid from stimulants, and with no untoward symptoms, she progressively recovered, and is now perfectly well. The catamenia have since appeared.

The success attendant upon the operation in this case is the best commentary on the advantage of prompt delivery, even under the most discouraging circumstances. She was so much exhausted, that it was not improbable that she might die under the operation. Had any further flooding taken place, we should probably have failed. Still as delivery afforded her the only chance, we determined to incur the responsibility, and the success was almost beyond our hopes. We were favoured, doubtless, by the mode in which the placenta was implanted. From the situation of the perforation I made, it is evident, that the os uteri was only partially covered by it; each successive dilatation leaving a larger space for the protrusion of the membranes, and at length allowing room for the extraction of the child, without any laceration of the placenta. This is not the only case in which I have found the good effects of laudanum after hemorrhage; given so as to produce an impression on the sys-

tem. Its mode of action is probably by reducing all the vital functions to the level of the circulating system, so that the demand shall not be greater than the supply.

The two following are cases of flooding after delivery, owing to the presence of a morbid growth within the uterus.

CASE III.—Mrs. Fagan, ætat. 45, a strong healthy woman, mother of two children; was delivered of the last, September 7th, 1833, after natural labour. The lochia ceased gradually about ten days afterwards. At six o'clock, A. M., September 21, 1833, on attempting to void urine, a gush of blood took place, filling the 'pot de chambre;' this was repeated before ten o'clock, the time of my first visit to her. I found her very pale and weak; no headach. Cool skin. Pulse 100; small but not very weak. The flooding had quite ceased, and I forbore making an examination per vaginam, lest I should reproduce the hemorrhage. The fundus uteri extended above the pubis, and was more bulky than is usual a fortnight after delivery. I directed cold to be applied to the vulva, and prescribed a grain of opium to be repeated in two hours.

At my visit the next day, (September 22), I found the draining had continued slightly, and her weakness somewhat increased. Pulse slower and weaker; she seemed fretful. A plug was introduced into the vagina, and I ordered two grains of the acetate of lead, with half a grain of opium, every hour.

September 23rd. More draining, accompanied by discharge of some clots. Pulse weaker. No effect seemed to follow the medicine. A dose of ergot of rye was now exhibited, but without any effect.

24th. Has passed no more clots. Feels very weak. Pulse 120; feeble. Mist. Ferri. Co. was ordered.

25th. As no benefit had resulted from the medicines administered, I suspected some further cause for the mischief, and I made a careful examination per vaginam. I found a round, smooth tumour, just protruding through the open os uteri. It felt soft and spongy. I could pass my finger round it, but not

sufficiently high to ascertain its insertion. Its size appeared about that of the larger end of a hen's egg. On examining the abdomen, I noticed the uterine tumour to be a good deal diminished in size.

Dr. Johnson, who visited her with my colleague and myself, was averse from performing any operation in her exhausted state, and recommended tonic medicines and a more generous diet. The plug was continued, but seemed to cause great vaginal irritation. From this time but little draining occurred.

My friend, Dr. Montgomery, saw her a few days afterwards, and at his suggestion, an injection of infusion of camomile with acetate of lead, was used twice or thrice a day, which lessened the irritation of the vagina, and the uterus gradually contracting, enclosed the polypus, so that it could no longer be felt, and the os uteri appeared perfectly natural. A thin mucous discharge continued for some days. The debility persisted a long time, but at length her strength returned, and she is now perfectly well.

I regret much that I did not make an examination, per vaginam, at an earlier period, although from the diminution in the uterine tumour, observed *the very day on which I discovered the polypus*, it is probable I might not have been able to reach the morbid growth previously. The hemorrhage here appears perfectly analagous to those described as resulting from uterine polypi, except that owing to the peculiar condition of the uterus after delivery, it was probably much more profuse; the effect on the system, doubtless, was much less than it would have been had it occurred sooner after her accouchment. From the most careful investigation I could make, I do not think that any of the means resorted to had much effect in diminishing the flooding, which appeared to subside gradually and spontaneously. I have, in other cases, found the patient considerably annoyed by the irritation of the vagina, arising from the presence of a plug. The interest excited by this case had scarcely subsided, when I was called upon to visit a patient in whom I had an opportunity

of contrasting the effect produced by a similar cause, acting during the progress and immediately after delivery.

CASE IV.—Mrs. Clarke, æt. 34, a strong healthy woman, mother of three children, was taken in labour at seven o'clock, P. M., January 1st, 1834. Presentation was natural, and the progress satisfactory. The head was expelled about 9 o'clock, accompanied by a profuse hemorrhage, causing faintness. A considerable interval elapsed before the expulsion of the body, which was followed by gushes of blood. The pupil who attended her applied cold to the lower part of the abdomen and vulva, and fixed on a tight binder with compress. I visited her at half past nine, P. M. The skin was blanched and cold; pulse rapid, and scarcely perceptible; hemorrhage had ceased; placenta not expelled. Removing the binder, I applied pressure by the hand to the uterus, and after a short time it contracted, though not firmly. I now removed the placenta, which was followed by some clots of blood: a series of compresses were placed on the uterus, and the binder tightened: some stimulants were given, and the patient appeared to rally: her pulse became more distinct and firm, and she said that she felt stronger: no draining. After remaining a considerable time, and no bad symptoms recurring, I left her in charge of a pupil, who staid some hours with her, and finding a gradual improvement, returned home, leaving directions with her attendants.

About four, A. M., January 2nd, she became rather feverish, and a great hemorrhage followed; cold was reapplied, the binder tightened, the acid mixture given, and an æther draught. The flooding subsided. I saw her at nine, A. M., the pulse was just perceptible and very quick; surface cool; eyes sunken, and an expression of great anxiety in her face; no draining. I gave her a scruple of ergot of rye, in decoction, without apparent effect. At twelve, A. M. I was again summoned; the flooding had returned; the uterus felt large and flabby; pulse imperceptible at the wrist; restlessness and anxiety. I gave her half a drachm of ergot, which was rejected by the stomach;

and in about half an hour, after a discharge of a great quantity of coagula, she expired.

As may be supposed, the circumstances of this case caused me great anxiety ; the favorable condition of the woman before delivery, the rapidity with which death followed, the signal failure of all the usual means of arresting uterine hemorrhage, notwithstanding their prompt application ; all conspired to make a post mortem examination a matter of great importance, and I was fortunate enough to succeed in my request for one.

We found the uterus larger than natural, of a pale colour, containing little blood in the sinuses, and the cavity apparently filled with coagula ; on removing these carefully, a morbid growth was discovered about the size of a duck's egg, attached by its broad extremity to the fundus uteri ; its tissue evidently continuous with that of the uterus ; its texture, loose and fibrous, was easily separable into distinct parts, without apparent blood-vessels, and with no continuous membrane covering it. A careful examination discovered no vessel from which the blood had escaped ; the placenta appeared to have been attached partially, at least, over the polypus. The preparation is in the museum of the Digges-street School of Medicine.

The resemblance of this case to the one recited is very striking, neither were preceded by any symptoms which could point out the probable occurrence of flooding : in neither was there any thing at the time which indicated an origin from organic disease, and in neither was any beneficial effect produced by the usual remedies : unfortunately, in the latter case, the circumstances under which the hemorrhage occurred led to a fatal termination. In both the tumour, as far as we could ascertain, was nearly alike in size, and communicated a similar feeling to the finger ; and in both, I should suppose, it was of recent growth. Why an apparently unorganized tumour should give rise to so profuse an hemorrhage, except by preventing uterine contraction, I confess myself unable to explain ; yet, had that alone been the cause, how can we explain the delay of a fortnight after

delivery in Mrs. Fagan's case. It is an extensively ascertained fact, that flooding is an accompaniment of polypus in the ordinary states of the uterus, but hitherto I have seen no explanation of its occurrence perfectly satisfactory.

The only remedy which a knowledge of this case would suggest, as being at all applicable to similar ones, is the exhibition of ergot, with constant manual pressure upon the uterus, on the one hand, and a plug introduced into the vagina on the other. I need scarcely mention, that the latter remedy, without the former, would be merely to substitute internal for external hemorrhage.

BIBLIOGRAPHIC NOTICES.

Notices concerning Works on the Practice of Physic, Surgery, Pathology, and Physiology, recently published in Germany.

(Continued from Vol. IV. page 424.)

THE following is extracted from the number of *Hecker's Literary Annals of Medical Sciences*, published in December, 1832, and contains so complete and instructive a view of the subject concerning which it treats, that I have been induced to give, not an abstract, but a literal translation of the whole paper; at the present moment, indeed, when the subject of the functions of the nervous system has excited renewed discussion, and the opinions advanced by Sir Charles Bell have again been submitted to the test of experiment, it is of great consequence that the labours of continental physiologists should be made known as speedily as possible to their English brethren. The analysis published in the January number of the *Medico-Chirurgical Review* of the experiments instituted by Professor Müller, of Bonn, has consequently been gratefully received by those interested in the progress of this important investigation, a circumstance which has confirmed me in the belief, that the labours of Dr. Stannius would excite a similar interest. From a perusal of the following paper, it will appear, that Dr. Stannius differs in some important particulars from Treviranus, and those who maintain that the nerves or the ganglia control and regulate the function of the circulation, a doctrine brought forward by some in explanation of the pathology of Asiatic cholera, and lately advocated in a well known medical society in England.*

* Liverpool Medical Society, *vide* *Medico-Chirurgical Review*, edited by Doctor Johnson, January 1834, p. 187.

Ueber einige Functionen des Nervensystems. Von Dr. H. Stannius, praktischen Arzte in Berlin.
On certain Functions of the Nervous System, &c. &c. &c.

SECTION I.

No great interval of time elapsed between the discovery, that sensation and motion depend on the nervous system, and the attempt to ascertain with accuracy by what portions of that system these functions are respectively performed. Thus Rufus of Ephesus* relates, that Eristratus distinguished between nerves of sense and nerves of motion, and that he believed the former to be hollow, and formed by the membranes of the brain, while the latter are derived from the substance of the cerebrum and cerebellum.

Galen† divided the nerves into hard, soft, and intermediate. The hard are destined for the function of motion, and the soft for sensation, while those of an intermediate consistence serve partly for one, and partly for the other purpose. The hard nerves he derived chiefly from the spinal marrow, the soft from the anterior part of the cerebrum, and the intermediate from the medulla oblongata. He believed that the posterior portion of the medulla oblongata gives origin to most of the motor nerves, and some few of sensation, while the proportion is reversed with regard to the nerves derived from its anterior half. Succeeding physiologists either assumed the existence of certain hypothetical differences of structure in the nerves themselves, and thus accounted for the difference of their functions in different parts of the body, or else they supposed that the functions of the nerves are modified by the structure of the organs to which they are distributed.

Treviranus‡ in his youth proposed the theory that sensation depended on the medulla of the nerves and motion on their neurofilema.

Charles Bell,§ in the year 1811, in a book circulated only among his friends, came forward with the following announcement:—

“ The difference observed between the functions of different

* De Corp. Human. part. appellat. lib. ii. p. 65, ed. Clinch.

† De Usu part. lib. ix. c. 14, p. 523, et alibi.

‡ Physiolog. Fragmente. Vergl. Biologie. Bd. v. s. 346.

§ An Idea of a new Anatomy of the Brain, submitted for the observation of the author's friends. Compare also, Romberg's Translation of Bell's Physiological and Pathological Researches concerning the Nervous System, Berlin, 1832.

nerves, depends on the difference of their origin from the brain and spinal marrow; every nerve with two functions possesses them in virtue of its being formed by means of a double root, &c."

In this essay he asserts that he had laid bare the roots of the spinal nerves, and had cut the posterior row of roots, without producing any convulsions in the muscles these nerves supply, while, on the contrary, the moment he touched the anterior roots with the point of his knife, these muscles were convulsed.

Bell's views do not seem to have been adopted at the period of their promulgation by any one except his son-in-law, Mr. Shaw,* and hence we can explain how it came to pass that John Cross,† in 1815, advanced the hypothesis that the anterior pillars of the spinal marrow are destined for sensation, and the posterior for motion. Alexander Walker‡ contested the originality of this discovery, and asserted that he had taught a similar doctrine before the publication of Mr. Cross's paper.

In 1818, Burdach,§ assisted by V. Baer, made a series of experiments on frogs, and arrived at the result, that the united influence of both the posterior and the anterior roots is necessary to produce in any spinal nerve, a perfect discharge of function, whether of motion or of sensation. At a later period, Burdach, having again repeated these experiments, thus expresses himself:—

"I saw that irritation of the posterior roots caused convulsions, and that their division produced relaxation and paralysis; while division of the anterior roots occasioned also paralysis, and that in a greater degree, while it also deprived the parts of sensation. After division of the posterior roots ensued debility bordering on or resembling paralysis; while after division of the anterior roots, ensued a complete extinction of the vital powers of the limb."

In 1822, Shaw communicated to Majendie the views entertained by Charles Bell, and Majendie¶ immediately commenced a series of experiments on the cerebral nerves, and on the roots of the spinal nerves. The latter seem at once to have led to the most decisive results. Thus in a young dog, having divided the posterior root of the spinal nerves, he says:

"The sensibility was always completely extinguished."

* Manual of Anatomy, 1821.

† Thomson, *Annals of Philosophy*, vol. v. (1815,) p. 112.

‡ *Ibidem*, p. 317, and vol. vi. p. 120.

§ Vom Bau und Leben des Gehirns, Th. 1, S. 263.

|| *Ibidem*, Th. 3, S. 400.

¶ Majendie, *Journal de Physiologie*, T. ii. p. 276, et seq.

When the anterior roots were cut,

“The limb became relaxed and motionless, while it evidently retained its sensibility.”

And finally he thus expresses himself at the conclusion of his essay :

“The posterior roots appear destined more particularly to fulfil the functions of sensibility, while the anterior roots are more especially connected with the function of motion.”

But in a second dissertation* upon the same subject, he says—

“These facts seem to establish the conclusion, that sensation does not derive its origin exclusively from the posterior nerves, nor motion from the anterior.”

Fodera† was the next to publish on this subject, but his results were still more inconclusive than those obtained by Majendie, and in some instances were quite opposed to them.

Bellingeri‡ assures us he has found that extension of the limbs depends on the posterior pillars and roots, while the power of flexion is derived from the anterior.

Schöps§ did not arrive at any very well established or satisfactory conclusions from his experiments on the mammalia.

Backer,|| whose essay I have not been able to obtain, is said to have been more fortunate in obtaining decisive results, and Beclard¶ speaks likewise of a similar success, but does not enter into the details.

Such was the state of the question when Joh. Müller** published an account of his experiments on frogs, from which it followed that Bell was perfectly correct in assigning the origin of motion to the anterior, and of sensation to the posterior roots.

Experiments performed in the presence of Scarpa by Pannizza,†† and by Seubert,‡‡ in the presence of Tiedemann and Arnold, are said to have led to results not less decisive. Less

* Majendie, *Journal de Physiologie*, t. ii. p. 367, et seq.

† *Ibidem*, t. iii. p. 191.

‡ *Bulletin Médical*, 1824, i. p. 299.

§ *Meckel's Archiv*. 1827.

|| *Comment. ad Quæstion Physiolog. Med. Acad. Rheno, Traject.* 1828.

¶ *Elém. d'Anatomie Génér.* p. 668.

** *Froriep's Notizen aus dem Gebiete der Natur und Heilkunde*, No. 646 und 647. März 1831. *Annales des Sciences Nat.* 1831. *Romberg's German Translation of Charles Bell's Views*, 1832, S. 375. *Meckel's Archiv*. 1832, Bd. 6, S. 67.

†† *Scarpa de Gangliis Nervorum de que Origine et Essentia Nervi Intercostalis*, ad H. Wererum *Epistola*. Mediolan. 1831.

‡‡ *Bischoff Nervi Accessor. Willis. Anatomia*, p. 79.

favourable to the new doctrines were the conclusions at which Gottfried Reinhold Treviranus arrived.

“ I observed, says he, in all the frogs on which I performed this operation, that life was extinguished much sooner, than it usually is in those experiments where all the viscera of the abdomen and chest are excised, while the spinal marrow remains untouched. They were attacked with opisthotonus the moment the air had obtained entrance into the spinal cavity, and, therefore, the motions observed to follow irritation of the posterior roots may be considered as automatic, and not as indicative of the animal's feeling pain.

“ No nerve of motion which enjoys an uninterrupted and continuous course from the brain or spinal marrow, to the external parts, is entirely destitute of sensation, and again we may suppose, that the nerves which are limited to sensation, are so limited only, because the parts they supply are incapable of motion.”

Although my own experiments confirm the conclusions arrived at by Müller, yet I am induced to publish them, partly on account of the comparative novelty, and the paramount importance of this physiological question, and partly because my experiments have brought to light some collateral circumstances of moment, tending to place in a strong light how careful we ought to be in pronouncing judgment upon the facts or opinions detailed by preceding authors.

My first step was to open with the greatest care the spinal canal, either by means of a very fine scissors, one blade of which was cautiously introduced into the canal, or by means of a forceps; I next proceeded to remove the layer which covers the spinal marrow, and which consists of a partly membranous, partly flocculent, highly vascular tissue. This done, I then attentively examined whether the functions of the nervous system were thereby impaired or injured. In more than an hundred experiments, I observed the opisthotonus described by Treviranus only twice; in all the other instances the animals leaped about as actively as if nothing had happened. Any hemorrhage which took place during the act of opening the spinal cavity, I easily stopped by dropping on the part a few drops of cold water, and after the operation was over, I used to leave the animal for a short time in a bowl of cold water. In this the animals moved about briskly, and exhibited their usual degree of sensibility, when any portion of their surface was irritated. When I divided the four or five posterior roots of the nerve which supplies one of the legs, the animal continued to move that leg just as energetically as usual, but did not exhibit the least symptom of uneasiness, either in the leg itself, or any other part of the body, when irritating applications, however strong, were made to that extremity, and in one case this

insensibility lasted for five weeks and a half after the operation, in another three. Mechanical irritation of the extremities of the divided roots, which were not in connexion with the limb, never produced the least motion in the latter. When the anterior roots of the nerve which supplies a leg were divided, while the posterior were quite uninjured, the consequence was, loss of both sensation and motion in the limb. The strongest mechanical irritation of the foot, its amputation, or the cutting off the leg itself, produced not the least disturbance on the part of the animal, provided the matter was so managed, that the hand of the operator was not seen by the animal during its approach to perform the operation. It is true, that frequently the frog moved either spontaneously, or from being disturbed by its body being shaken during the experiment, or from the agency of some other accidental cause, but in the course of fifty experiments, I only twice witnessed motions that could be set down to the account of stimuli directly applied to the limb. The experiment was always conducted with the greatest caution, and especial pains were taken not to bruise, stretch, tear, or otherwise injure in the slightest degree the posterior roots, and yet I always obtained the same result!

Doctor Gädeckens, and Doctor Henle, witnessed my experiments, and expressed themselves satisfied with the correctness and accuracy with which they were performed, and their testimony was the more valuable, because one of these gentlemen, Dr. Henle, had been constantly present at Müller's experiments. In truth, I was almost forced to conclude, that Müller had committed some oversight, and I was nearly convinced, that the posterior roots were capable of transmitting sensation from the limb, only so long as they remain in living contact with the anterior, which preside over motion.

I now resolved to keep one of the frogs for some time after the operation, and with that view I preserved the animal in a glass of water. On the third day I took him out, and observed, that when I pinched or otherwise irritated the foot of the leg, the anterior roots of whose nerve had been divided, that the animal exhibited indubitable proofs of feeling the irritation; the next day the motions in the rest of its body which followed the irritation of the same foot, were still stronger, and on the sixth day, matters had so far altered from what I had observed immediately after the division of the anterior roots, that the sensibility of the foot and leg seemed, if any thing, greater than natural, for the animal could not bear these parts to be touched in the gentlest manner, without evincing symptoms of the greatest uneasiness. I preserved this frog alive for five weeks longer, and at the end of that time I could not perceive the slightest

indication of the limb having regained its power of motion. Similar or nearly similar results were uniformly obtained, and I could not help feeling strongly, that my conclusions still differed very materially from Müller's, for he had observed, that sensation was not impaired immediately after the operation of dividing the anterior roots. At last, however, a combination of circumstances led me to form the opinion, that the sensibility of the limb, in my experiments, had been impaired by the cold water I used to stop the hemorrhage. Accordingly, I again repeated the experiment, using a sponge very slightly moistened to remove the blood, and avoiding pouring cold water on the spinal marrow, or placing the animal in cold water. When I had adopted these precautions, I obtained in eleven successive experiments the same results precisely as Müller; thus after division of the anterior* roots, the extremity altogether lost its power of motion, but when any part of that limb, as for instance its skin or toes, were mechanically irritated, the animal displayed indubitable symptoms of the irritation being felt, for it instantly became uneasy, and moved the head and three other extremities in the manner it is accustomed to do when in pain.

When I placed a frog thus operated on in water, it still displayed evidence of feeling any irritation applied to the limb rendered destitute of motion. The apparent discrepancy between Müller's experiments and mine cannot, therefore, be attributed to any power which water in itself possesses of acting specifically in such a way as to diminish sensibility; the difference in our results may be more satisfactorily explained by the peculiar effects produced by cold water, when it is dropped or poured upon the exposed spinal marrow, for in my experiments the shock thereby imparted, *had for the time deprived the posterior roots of their power of feeling*: mechanical irritation, applied to the posterior roots of the nerves cut off from the spinal marrow, had never the least effect upon the frog.

On the whole the following results may be derived from my experiments. 1st. The posterior roots have no power of conveying impressions from the centre to the periphery, they are not capable of exciting motion, nor do they possess the power of gradually assuming that function when the proper nerves of motion have been divided. Their proper and peculiar office is to convey the sensation of impressions made at the periphery to the centre of the nervous system. 2nd. The anterior roots, on the other hand, enjoy the privilege of being the conductors of volition, and convey impressions from the centre

* In the text it is *posterior*, evidently a typographical error.—Translator.

to the circumference; in a word, they preside over muscular motion.

In the detail of Fodera's experiments, many circumstances are related which seem to indicate, that the reverse holds good in the anterior extremities, but these circumstances have been explained by Müller, who has clearly proved, that the several roots of the spinal nerves perform the same functions in these as in the posterior extremities. It is true, that when the posterior roots are divided, the frog moves its arms in an awkward manner, and seems to possess less power of grasping bodies, nor in the sequel does this power appear to be recovered. Still, however, a considerable power of motion remains. It is worthy of remark, that in two out of twenty frogs, in whom I had divided the posterior roots of the nerves which supply the arms,* a temporary loss of motion accompanied that of sensation, although no injury had been apparently inflicted on the anterior roots. In both cases, however, the power of motion was restored in about half an hour. When the anterior roots of the nerves supplying the leg of a frog have been divided, and that leg is then irritated mechanically, a twofold reaction is observed in the animal, viz., an attempt to move or jump with the other leg, and with both arms, and a sudden motion of the whole body, including the head; this latter motion appears convulsive, as if the animal suddenly shuddered from fright. I have not been able to perceive, that either of these motions occurs antecedently to the other. Still, as one is evidently a combined movement, destined as it were to perform some given object, a character which the other wants; there is little doubt, that they depend upon the agency of different portions of the central organs of the nervous system.

This supposition is strengthened by the following experiments. 1st. Having carefully opened the head and spinal canal of a frog, and divided the anterior roots of the nerves supplying one of the legs, I next cut the spinal marrow across, where it arises from what is in the frog termed the cerebellum. I then removed both cerebrum and cerebellum. When mechanical stimuli were now applied to the leg thus deprived of its motive powers, the other leg was merely drawn towards the body suddenly, with more or less strength; when the irritation was increased, both arms were likewise moved as if in the act of preparing to leap. In this experiment the shuddering, convulsive motion was altogether wanting. 2nd. When after the division of the anterior roots of the nerve supplying one leg, I

* For brevity sake, I translate in the case of frogs, the anterior extremities arms, the posterior extremities legs.

cut the spinal marrow across, just above the origin of the nerves of the posterior extremities; then the experiment never failed to produce a drawing up of the other leg towards the body, as if it was performing the motion necessary for leaping, and this took place, not merely when the leg was uninjured, but when the posterior roots of its nerve had been divided. From this it follows, that when an irritation, acting upon the nervous extremities, gives rise to motions in other and distant parts, this is done independently of the agency of the brain and will. The sudden shuddering of the whole body, which never took place where the brain had been removed, may with great probability be ascribed to a painful sensation. These experiments likewise prove clearly, that when a leg, deprived of its proper power of motion, is made to move by the application of a stimulus, it does so in consequence of the pain thus felt, and not from other causes.*

SECTION II.

Most physiologists are of opinion, that *sensibility* exerts an immediate influence on the circulation, even independently of its influence on the heart. Many indeed go so far as to assume, that the local circulation of any part must cease, when that part is deprived of the active influence of its nervous branches. *Burdach* seems to have adopted this view, and *Treviranus* defends it in the following terms.

“ 1st. Division of the main nerve which supplies any limb, is immediately followed by the loss of sensation and motion. The circulation in the limb continues for a certain time, when it ceases, and the limb dies, unless indeed, as sometimes happens, it is saved by the reunion of the extremities of the divided nerve. Thus it happens to the leg when the ischiatic nerve is divided, and similar effects are observable in the circulation of any part where the spinal marrow is cut across above the origin of the nerves supplying that part; the latter operation, however, produces in addition considerable effects on the general circulation. These facts, continues *Treviranus* in another part of his celebrated *Biologie*, prove, that each portion of the spinal marrow presides over the circulation of the organ it supplies with nerves. This it does too, quite independently of any agency it may exert on the heart, for the general circulation continues undisturbed after all motion of the blood has ceased in the limb, whose connexion with the nervous system had been cut off. The circulation does not instantly stop in a limb when its nerves are divided, for this reason, that every nerve retains enough of energy to maintain

* The author remarks, that when the *medulla spinalis* or *oblongata* is cut across, the frog immediately appears lifeless, but after some time recovers.

the circulation for a certain time after its own communication with the cerebro-spinal system has been interrupted. As it is now insulated, and obtains no fresh supplies of nervous energy, its power of maintaining the circulation necessarily soon ceases."

Towards the end of the second volume of his *Biologie*, Treviranus details the experiments on which he founded this doctrine. They are as follows:—When both the ischiatic nerves of a frog were divided, the circulation of the blood in the webs of the feet ceased in the space of two minutes. In another frog this operation was followed by a great diminution in the energy and quickness of the circulation in the foot; but it continued in this enfeebled state for some time. When the spinal marrow was cut across in the middle, the motion of the blood in the feet continued a full quarter of an hour after the operation, without becoming notably slower. In another frog the circulation of the blood ceased at once in the legs when the ischiatic nerves were divided. In a full grown and very lively female frog, the circulation in the feet lasted ten minutes after the spinal marrow was divided in its middle part. When examined after the lapse of eight hours, all motion of the blood had ceased in the larger vessels, but still continued in the capillaries. In another frog the circulation in the leg continued more than half an hour after the division of the ischiatic nerve. Such are the facts upon which Treviranus founded his opinion, and fully relying on which, I was induced to make some experiments with the view of determining whether the anterior or the posterior roots of the nerves exert the greatest influence on the circulation. But I could not detect the least alteration in the circulation of the animal's leg, either when I divided all the anterior roots, or all the posterior, or both. *The circulation did not appear to be disturbed, either immediately after the operation, or during the course of five weeks.* It is to be observed, that I kept the frogs so experimented on, in a glass containing water enough to cover every part of the body, but the head and back. Frogs also which were kept, but not in water, for several days after the operation, exhibited no derangement of the circulation of the blood, either in the anterior or posterior extremities. Having cut the spinal marrow across, just above the origin of the nerves of the legs, and having moreover cut away a little bit from the end of the anterior portion, in order to prevent all possible communication between it and that connected with the legs, I observed in about ten cases a sudden cessation of the circulation, immediately after the operation, in the webs of the feet. In such cases the frog was invariably in a state of apparent lifelessness, being deprived of

sensation and motion, not merely in the extremities below, but in those above the incision. Sensation and motion were restored in the anterior extremities, sometimes so speedily as in three minutes, occasionally not before twelve. Somewhat later the legs also were acted on by stimuli applied to their surface. The circulation in the vessels of the web ceased in general for only a few minutes, and never for more than twenty, after which it recommenced but in a more sluggish manner than before the operation. When I applied salt to one of the feet, I observed an immediate efflux of blood to the part, and the web exhibited speedily an injection of all its capillaries with red blood. In ten cases the operation above described produced no obstruction whatever in the circulation of the feet. One frog, whose spinal marrow had been thus cut across, and a bit excised, I preserved alive for eight days, without being placed in water, and the animal no doubt would have survived much longer, had I not submitted it to another experiment. In this frog, any mechanical irritation of either foot produced immediate retraction, not merely of the corresponding, but of the other leg. The blood too circulated rapidly and evidently in the vessels of the web, and on the eighth day blood flowed in abundance from a wound inflicted on one of the thighs.

Two other frogs in whom the spinal marrow was cut across, and a bit excised, were placed in a vessel of water. One of them ceased in six hours and a half, and the other in eight hours, to betray any symptom of re-action on the application of stimuli to the toes, or the skin of the legs. In both, the circulation of the blood in the vessels of the webs of the feet continued for six days, when they were killed. In two full grown female frogs, I divided *all the nerves* which the posterior extremities derive from the spinal marrow, and to prevent all further communication, I excised the portion of the spinal marrow which gave them origin. No re-action of the legs took place on the application of stimuli to their surface. The circulation of the blood in the webs of the feet went on without interruption. I placed both the animals in a vessel of water, and observed them for five weeks. Sensation and motion did not return, but the circulation of the blood remained unimpaired. The posterior extremities did not undergo any evident diminution in size, but secreted from their surface (in much greater quantity than the anterior) a thick, clammy mucus, which I frequently found in large quantity on the skin of these parts. In all the experiments I had hitherto made, I had divided the roots of the spinal nerves before the formation of the spinal ganglion, and, consequently, before they had formed any communication with the sympathetic. I next opened the abdominal cavity in two frogs,

and divided within it, all the nerves supplying both anterior extremities in one, and in the other those only which supplied one leg. I then carefully settled the displaced viscera, and stitched up the abdominal parietes. No great vessels were wounded during the operation. The blood continued to circulate in the webs of the feet, although somewhat languidly, immediately after the operation. They were both placed in water, and observed on the following day, when the circulation was found to continue in the same state. The frog, both of whose legs were paralysed, died on the day but one after, while the other survived eight days, during which no cessation of the circulation in the affected limb could be detected.

I have frequently succeeded in dividing the ischiatic nerve, without injuring any considerable blood vessel ; in some of these experiments this division was followed by a temporary arrest in the circulation of the corresponding foot and its webs, but this continued only for a few minutes; after which the circulation was restored. In general, this operation did not produce any stoppage of the circulation in the limb. *Treviranus* would undoubtedly have arrived at similar results, had he kept the animals on which he experimented, a longer time after the operation, for the purpose of observing its effects. Yet he too found on one occasion the blood circulating in the smallest vessels of the foot, eight hours after the operation.

It follows from the experiments I have detailed, *that neither the spinal marrow, nor any of its nerves, before or after their connexion with the sympathetic, have such an influence on the circulation of a limb, as to occasion it to cease, when all communication between that limb and the rest of the nervous system has been cut off.* When the circulation becomes weaker in a limb, whose nerves have been divided, this occurrence is to be ascribed partly to the loss of blood during the operation, and partly to the constant state of rest in which the paralysed limb necessarily remains, in consequence of which, it naturally attracts less blood to itself. The more energetic and repeated are the exertions of any organ, the greater the quantity of blood it requires and obtains from the general mass of circulating fluid ; hence less blood flowing to paralysed parts they become emaciated, and in some cases a perceptible diminution in the calibre of their main artery takes place. But what caused the sudden stoppage of the circulation which *Treviranus* observed so often, and I occasionally ? It cannot be too often repeated, that the animal system is an undivided whole ; every component part may re-act not merely on the organs with which it is more immediately connected, but on the whole frame, and when any part is of great consequence in the sys-

tem, its structure cannot be injured, and its functions destroyed, without a great derangement in the whole system being a necessary result. If the injury be inflicted suddenly, this result is obtained with more certainty, and to a greater degree. In this manner I think we can account for several facts observed in experiments similar to those above detailed, and it is to the shock received by the system at large, in consequence of the sudden cutting of the spinal marrow, that we are to attribute that complete and general loss of sensation and motion, which takes place and lasts for a few minutes after this operation, and to this general shock also must we attribute the cessation of the circulation already spoken of. In conformity with this view of the question, we can explain why the circulation stops more frequently, and for a longer time, after division of the spinal marrow, than after that of the ischiatic nerves.

SECTION III.

I opened the spinal cavity in several frogs, cut the marrow across just above the origin of the nerves which supply the legs, and then removed the brain together with the whole of the spinal marrow, above (anterior to) the point of division. The anterior extremities, whose central nervous system was thus destroyed, were totally deprived of sensation and of motion, on the application of stimuli to the periphery of the nervous system. When the anterior roots of the divided nerves were pricked, the limb which they supplied was convulsed, when one of the toes of the posterior extremities was irritated, a sudden contraction of the whole leg was the consequence. These motions sometimes took place at the same moment in both legs, and then much resembled those which are made preparatory to leaping. Irritation of the skin around the anus was the most powerful means of exciting these motions of both the legs. One of these frogs which was left in a china cup without water, *re-acted** during forty-eight hours after the operation, whenever its toes and the skin of its legs were irritated. In other individuals the power of re-action continued for fifty-three, sixty-two, seventy-three, and seventy-five hours, respectively; when irritation of the toes had ceased to procure any symptom of re-action, it was often obtained by tearing the skin of the thigh. And when the nerves belonging to this part also had lost their conducting power, motions were occasionally obtained by applying the irritation to the parts in the neighbourhood of the

* By *re-acted*, is obviously meant, *shewed symptoms of irritation, such as convulsive movements, &c.*

anus. After all power of conducting impressions made on the nervous extremities to the spinal marrow had ceased, I opened the thoracic cavity in the animals referred to, and generally found the heart still beating; when this was the case its contractions continued in some instances as long as two hours after opening the chest. In the individual where the power of reaction continued seventy-three hours, the heart was found empty, and yet it contracted when irritated with the point of the forceps.

In general it may be remarked, that every mechanical irritation of the anterior roots of the nerves produced contractions of the muscles which the nerves corresponding to these roots supplied, for several hours after the power of sensation had altogether ceased in these parts. The pulsating organ which J. Müller discovered,* and which is situated by the side of the anus, continued its contractions, seven, eight, and in one case so long as nine hours after the heart had ceased to beat. The organ on one side was often observed to continue pulsating, after that on the other side had become altogether motionless. Its longest diameter did not invariably correspond in direction with that of the animal, for I saw it in several frogs longest transversely, and in these cases the contractions of the organ proceeded from without, inwards. All the phenomena hitherto described occurred when I placed the animal, deprived of its brain and one half the spinal marrow, in a vessel without water, and in which it was exposed to the direct contact of the air. But when a frog so operated on was placed in a vessel with cold water, the nerves of the posterior extremities lost their power of producing motions when their periphery was irritated, in the course of from five to eight hours. The resulting phenomena were the same, no matter whether the animal was totally immersed in water, or was merely placed in it, but in such a manner that it did not reach to its back and spinal marrow. Mechanical irritation of the motor nervous roots continued to produce contractions, after all conducting power from the periphery to the centre had ceased, but in a very short time even this symptom of nervous influence disappeared. This peculiar effect of cold water shewed itself also in those cases where I merely cut the spinal marrow across, and then placed the frog in cold water. Sensation and motion continued undisturbed in the anterior extremities. The posterior re-acted, on the application of stimuli to their surface, for six or even nine hours, after which, in three instances, every trace of sensibility to cutaneous irritants had dis-

* Poggendorf's *Annalen für Physik und Chemie*, 1832, a. 517.

appeared. The circulation of the blood, nevertheless, continued in the webs of the feet. Two of these frogs, which I kept for six days in water, did not in that time recover the power of re-action on the application of stimulants to the posterior extremities. Another frog was taken out of the water at the end of the third day, and having been carefully dried, was placed upon a saucer. The capability of receiving impressions from the application of stimulants to the legs, had not returned after the lapse of twenty-four hours; even the actual cautery, applied to the toes, produced no appearance of convulsions. I regret that I neglected to note the state of the circulation in the vessels of the feet in this animal. Whence arises the deleterious influence which cold water exercises on the nervous functions? is it to be attributed to the cold? This supposition is invalidated by the fact, that the same effects followed the application of water which had been kept in the room for several days, and which, consequently, had attained the same temperature as the air. *Alexander von Humboldt* long since remarked, that an organ which lay in cold water for four hours, had become much less irritable than a similar organ exposed to the air, provided that neither the water nor air were above eighteen or twenty degrees of temperature, (*Reaumur*). He is inclined to attribute the debilitating effects produced by the water to its washing the blood out of the part.

I feel more disposed to adopt the opinion of Nasse, who attributes the effects produced by water on parts still covered with epidermis to its preventing the free access of air to the surface. In corroboration of this, I may remark, that when I placed a frog, deprived of the brain and the anterior half of the spinal marrow, in oil, the animal lost every trace of feeling in the course of half an hour, although before immersion, irritation of the toes produced violent re-action in both legs. The sensibility continued somewhat longer, when I strewed powdered glass, charcoal, or sulphur over the legs of frogs deprived of the brain and half the spinal marrow. But when these substances* were merely sprinkled on the legs, so as to allow some access of air, the limbs retained their sensibility longer. When these parts were covered with common salt, the legs were immediately thrown into the most violent convulsions, and per-

* I purposely made use of some substances belonging to the class of conductors, as well as others which belong to that of non-conductors of electricity—no difference of result was however obtained. One frog, which lay on a thin iron plate, and whose spinal marrow was connected with a thin piece of iron, retained its irritability for sixteen hours, which is about the length of time it would have retained its sensibility if placed on glass or porcelain.

formed the motions used in leaping, while the muscles became stiff and hard, and the webs of the feet exhibited a most minute injection of their vessels with red blood. The sensibility, however, was soon exhausted. I must here observe, that common salt produced these marked and violent effects in only three cases; in some later experiments its effects were not near so striking. Many experiments upon the effects of warm water, led to the very same results as those instituted by our celebrated Humboldt.

ROBERT J. GRAVES.

Cyclopædia of Practical Medicine.—Part XIX. London, December, 1833.

THE credit which is reflected on British medical literature by this important publication, is decidedly enhanced by the late numbers which have appeared. On a future occasion, when the work shall have terminated, we trust to render it justice, more adequate to its merits than we have been enabled hitherto, by a full review of it. The part for December, contains several articles of the greatest importance, executed in a manner which is highly creditable to the work and to the authors. At present we select one for notice, with the object of laying before our readers a summary of some recent researches on the pathology of cancerous formations, a subject treated by Dr. Carswell, under the head of Scirrhus.

Richerand's observation, that cancer is not less difficult to define than to cure, is just as true at the present day, as when it was made, and in fact is rendered only still more obvious by the advance of pathological anatomy, which has shewn its specific relations to be more extended. Apparently under this impression, Dr. Carswell judiciously waives the definition, and having also forsaken his leading term scirrhus, he supplies in its stead certain descriptive characters which belong to cancerous products in general, all of which he groups under the generic denomination of *carcinoma*. They are the following: 1. They are essentially composed of a solid or fluid substance, different from any of the solids or fluids which enter into the healthy composition of the body. 2. They often present in the early periods of their formation certain characters common to all of them, however much they may differ from each other in the subsequent periods. 3. They all terminate in the gradual destruction or transformation of the tissues which they affect. 4. They have all a tendency to affect successively or si-

multaneously several organs in the same individual. 5. They all possess, though in various degrees, the same reproductive character. Such are the characteristics common to scirrhus, cancer, fungus hæmatodes, and the several kinds of sarcoma, and which justify these morbid formations being considered as having a generic relation.

Dr. Carswell has seized upon a most important pathological element, for dividing these products into species. It is found, that some amongst them have no organizable power within themselves, and seem to derive their nutrition, form, and arrangement, entirely from the tissues in which they are developed. Others, on the contrary, are organizable; although *at first*, their form and arrangement may be in a great degree determined by external circumstances, yet they possess independent properties by which their subsequent development is effected. This then permits them to be separated into two species, to the first of which Dr. C. has given the name of *scirrhus*, and to the second that of *céphaloma*; and under either of these heads may be arranged all the varieties of these formations. We subjoin here a view of them thus classed, with the denomination given to them by authors.

SCIRRHOMA, (<i>unorganized</i> .)	CERPHALOMA, (<i>organizable</i> .)	
Scirrhus, <i>properly so called</i> .	Common vascular, or organized Sarcoma, (<i>Abernethy</i> .)	
Pancreatic Sarcoma, (<i>Abernethy</i> .)	Mammary Sarcoma, (<i>Abernethy</i> .)	
Lardaceous Tissue, (<i>French Pathologists</i> .)	Medullary Sarcoma, (<i>Abernethy</i> .)	
Colloid matter, (<i>Laennec</i> .)	Cerebriform or Encephaloid matter, (<i>Laennec</i> .)	
Gelatiniform Cancer, (<i>Cruveilhier</i> .)	Spongoid Inflammation, (<i>Burns</i> .)	
	Milk-like Tumour, (<i>Monro</i> .)	
	Soft Cancer, (<i>various authors</i> .)	
	Pulpy Testicle, (<i>Baillie</i> .)	
	Fungus Hæmatodes, (<i>Hey</i> .)	
	Fungoid Disease, (<i>Sir Astley Cooper</i> .)	

These *specific* characters are very important in a physiological point of view; but it is to be stated, that as yet we have only a glimpse of their existence, and are not warranted in drawing any decided inference from them, as the capacity of organization is very variable, and is affected by conditions which we cannot apprehend. Besides, the well known observation is quite founded in fact, that some of the varieties pass into each other, of which the best established example is furnished by the unorganized scirrhus, which is frequently known to change successively into the state of medullary sarcoma, and fungus hæmatodes. The distinction into *varieties* is, notwithstanding, by no means to be overlooked, constituted as it is by

marked difference in physical characters, (which are unnecessary here to be described,) and what is still more essential, by different degrees of rapidity in their development, and in their reproduction.

In searching into the seat, origin, and the mode of formation of carcinoma, it is in the first place to be observed, that it is found in three situations: 1st, in the molecular structure of organs; 2ndly, on the free surface of membranes; 3rdly, in the blood.

The minute examination of the earliest appearance of carcinoma in a glandular structure, such as the liver, affords a most interesting insight into the manner in which this substance is deposited. It may be observed in a single *acinus*. This can be seen to change gradually in colour, while its *form* and *bulk* remain *unaltered*. It becomes in fact *transformed* by the molecular deposit of this peculiar substance, in lieu of its proper tissue, and the frequent examination of it leads to the persuasion that this takes place by its separation from the blood, in the same way in which the ordinary nutrition of the organ is accomplished.

The study of its production on serous membranes is also very instructive. Here it is to be regarded as a modification of secretion, but the difference between the latter function and that of nutrition is only nominal. In our present state of knowledge, at least, we have no means of distinguishing them except by their results, and the investigation of these teaches us, that the essential part of both these processes consists in the separation from the blood of elements already existing in that fluid. Several observations of modern pathologists tend to establish the important fact, that, occasionally at least, the products of disease are no exceptions to this order of secretion, instances of which will recur to the reader, in the cases where coagulable lymph, pus, tubercular matter, urea, &c. have been found in the blood. None of these are, however, of such interest as the facts with respect to carcinoma.

We have now to mention the existence of carcinoma in the blood, which appears to us to be the most important part of Dr. Carswell's article, and which we have only to regret he has not devoted more space to illustrate.

“ The following facts may be adduced as furnishing strong evidence that the formation of the carcinomatous substance takes place in the blood, whether it be found in this fluid alone, or in other parts of the body at the same time. First, the presence of this substance in the vessels which ramify in carcinomatous tumours, or in their immediate vicinity; secondly, in the vessels of a portion or of the whole of an organ, to the former of which the carcinomatous substance is exclu-

sively confined, and can be traced from the trunks into the branches and capillaries; thirdly, in vessels having no direct communication with an organ affected with the same disease, as, for example, when it is confined to a small extent of the vena porta; and, lastly, in blood which has been effused into the cellular tissue, and on the surface of organs.

“The appearances which the carcinomatous matter presents in the blood are very various: sometimes they are perfectly similar to those which mark its presence in the substance or on the surface of organs. When contained in large veins, such as the vena portæ and its branches, the emulgent vein, &c. it may present the lardaceous, mammary, medullary, or hæmatoid characters all in the same venous trunks. These varieties of the disease may be found mixed together in minute qualities, or isolated into masses so conspicuous, that we can readily distinguish them from one another. Sometimes they lie merely in contact with the internal parietes of the vein; at other times they are united with these by means of a thin layer of colourless fibrine; or minute blood-vessels pass from the one into the other, and are often very numerous and remarkably conspicuous in the cerebriform matter. The divisions of the vascular system, in which the carcinomatous substance has been observed, are the venous and the capillary, a circumstance which may be ascribed to the contractile power of the arteries preventing, under ordinary circumstances, the blood from accumulating, and consequently this substance from forming within them, and not to any peculiarity of function exercised by the former. The presence of the carcinomatous matter in the veins *might be*, and indeed *is in general supposed* to be owing to its having been absorbed by these vessels; but without entering into minute anatomical details which disprove such to be the case, we shall again repeat the fact, that there are cases of carcinoma in which the venous blood alone is found to be the seat of the disease. There is no accumulation of the carcinomatous matter either in the vicinity of the veins in which the blood is thus affected, or in any other part of the body; and in those cases in which this complication exists, as well might we refer the presence of this matter in the veins to the exercise of the function of venous absorption, as that of cellular, fibrous, osseous, and cretaceous formations in the same vessels to a similar process, which we know not to be the case.

“From this view of the origin of carcinoma in the blood, its formation in the ultimate structure, and in the free surface of organs, after the manner of nutrition and secretion, follows as a natural consequence. The material element of the disease is separated from the blood, and deposited under a variety of circumstances which modify in a greater or less degree the form, bulk, colour, and consistence which it afterwards presents, in the several periods of its development. We cannot, therefore, limit the seat of the disease to any one tissue, or ascribe its origin to any modification of structure or special organization, as has been done by several pathologists.”

We have quoted at length the words of the author in giving his account of carcinoma in the blood. The facts are of exceeding interest, and the arguments ingenious. We must confess, however, that they would have appeared equally strong to us, were not the conclusions stated in so positive a manner as the last paragraph conveys, for although the evidence that carcinomatous matter is found in the blood is irrefragable, and we think equally so that it is separated from it by the natural process of nutrition or secretion, we cannot but think it *premature* to say that it *has its origin* there, and to draw deductions from this as an established fact. We are by no means unfavourable to it, as a speculation, but we think the time is not yet come for putting it forth as a doctrine. The following summary observations, contained in a latter section of the article on *the causes and nature* of carcinoma, are unobjectionable, and seem to us to be more in the cautious spirit of this able pathologist.

“ We must therefore conclude, that if carcinoma make its appearance in individuals, however they may have been placed in regard to the operation of what are called predisposing and exciting causes, there must be present, in such individuals, a peculiar condition or disposition of body, previous to the manifestation of the disease. In what this disposition consists, we are entirely ignorant. Its very existence is made known to us only by means of the peculiar characters of the products to which it gives rise. We have certainly seen that the great function of nutrition is that which is more especially affected, and that, as the material element of carcinoma is contained in the blood, it is *highly probable* that a modification of this fluid constitutes the primary if not the essential condition of the disease. But, unacquainted as we are with the nature of nutrition as a physiological function, we can offer no explanation of the nature of the change effected in the blood, nor, consequently, of the nature of carcinoma.”

In treating of the form, bulk, colour and consistence which carcinoma presents in the different tissues and organs of the body, and in the several periods of its development, Dr. Carswell, has given a minute and very interesting exposition of the conditions which modify these physical characters. These, however, we must leave unnoticed, as our object here is to advert to subjects more immediately connected with its pathology.

Those of our readers who have given attention to this subject are aware, that the anatomo-pathologists of the present day have not hesitated to assert that scirrhus and cancer are to be considered merely as hypertrophy and induration of the cellular tissue, and that this opinion has been especially advocated by

Andral.* With the scalpel in hand, it is frequently the case, that no other anatomical element can be detected at the commencement of these formations than a firm, pale, compact, celluloso-fibrous-looking tissue, and to this the name of *hypertrophied cellular tissue* has been assigned. Ever since we have considered this subject closely, we have regarded this doctrine as unwarranted by analogy, and as an abuse of pathological anatomy. We are now glad to find that the same view is taken of it by so eminent an anatomist as Dr. Carswell, who shews that real hypertrophy of the cellular tissue, such as it is found in elephantiasis, and other affections, have no tendency to terminate in carcinoma.

“ Besides, admitting that a certain degree of hypertrophy may precede the presence of carcinoma, the facts already brought forward in illustration of the mode of formation of the disease, its seat, and origin, clearly shew, that no such change is necessary, inasmuch as all the varieties of carcinoma may form in situations in which the cellular tissue is either extremely small in quantity or does not at all exist. What, therefore, appears to be hypertrophied cellular tissue must be regarded a tissue *sui generis*, produced by the uniform distribution and molecular deposition of the carcinomatous matter, either in the cellular tissue of an organ, or in an accidental tissue of a similar kind, formed at the same time, and deposited along with the carcinomatous matter. Such is, in fact, the manner in which the cellular and fibrous tissues which enter into the composition of the carcinomatous matter are generally formed.”

There exists much variety as to the quantity of blood-vessels in the different species of carcinoma, they are rarely perceptible in any of the varieties of scirrhus, and seem to consist, in this species, of branches from the tissue in which it occurs, which have been enclosed in the deposit. It is in the cerebriform matter that they are most numerous, constituting often the greater portion of the tumour. In this variety they appear to arise in the substance itself, apart from the tissues in which it is developed. These most minute divisions terminate by penicillated extremities in the carcinomatous matter, where they communicate with veins and arteries belonging to the affected organ. The latter vessels may be said to form the collateral circulation of cephaloma, and are seldom so numerous as the proper vessels. There are cases, however, in which it forms the greater part of its vascular structure, and they are of the greatest importance, as the nutrition is supplied by them, and the destruction of the tumour is frequently caused by an

* Clinique Medicale, tom. iv. p. 404. Anatomie Pathologique, tom. i. p. 58.

obstruction in this part of its circulation. Mortification, either partial or total, of carcinomatous tumours is not an unusual occurrence from pressure of one mass on another, or from the constriction which the peduncle or base occasionally suffers when the tumour has passed through an opening in a fascia, or in the skin. Similar consequences occur, when it acts a local stimulus, causing congestion. According to the degree of the latter it produces at one time hemorrhage, at another softening, and at a third sloughing.

We must here close our remarks. They are digested from a consideration of a small proportion of Dr. Carswell's article, the remaining topics of which are treated with not less ability than that to which we have adverted. The whole shews the industrious and learned pathologist, and contains a body of profound and original research rarely indeed met with in medical writings of any description at the present day, much less in a cyclopædia. Criticism would be a bad trade, were authors to be thus enrichers of medical science by their own investigations and discoveries. *We* should then have to doff our judicial cap, and sit down and learn at their feet, or else be content with asserting our dignity by grave recriminations of our author's style. There is no reason for fear, however, at present, that we shall have to lament our "occupation gone," from such an unnatural revulsion.

We perceive that it has been impossible to embrace the whole work, as was originally intended, in twenty numbers. We congratulate the public, however, that the editors have not been tempted to crimp any of the articles into a smaller space than their authors thought fit, for the purpose of comprising it in the number they had specified. This would have greatly detracted from the work, and would have been an injurious want of confidence in the subscribers. We are sure that in expressing our satisfaction that it is to be extended to twenty-five numbers, inclusive of a supplement, we are only speaking the feeling of the subscribers, and of all those who wish well to this undertaking, so creditable to medical literature.

JAMES HOUGHTON.

Medical Periodicals in Germany.

THE following catalogue of German medical periodicals has been made in compliance with the wish of several subscribers, who are anxious to know the titles, prices, and characters of the principal journals published in Germany. In Germany, as in

Great Britain, the number of these publications has of late years greatly increased, a circumstance resulting, in the opinion of some, from the increasing vigour and growth of medical science, but considered by others as a proof of the baneful predominance of superficial knowledge, combined with a restless love of novelty. Instead of attempting to explain the causes which have produced the present state of medical literature, or endeavouring to expose its disadvantages or its merits, let us rather use every exertion to derive from that system useful and practical information, and let us strive to profit by things as they are ordered, in preference to making useless efforts to effect a change.

In the year 1829, the number of journals connected with medicine and surgery, published in Germany, amounted to thirty, and in 1832, to forty-one; since which, several new periodicals have been commenced, and at the present moment probably not less than fifty are published in the German language alone. It is a matter of some interest to compare the number of periodicals with that of the other medical publications. Now, as we have already stated, the periodicals were in 1832 forty-one in number, and from the catalogues published in the same year, it appears, that all the other publications on medicine, &c. including new editions, amounted to 370, so that the proportion of journals to other medical publications is as one to nine; when we take into account, that several of these periodicals are published weekly, and that few, with the exception of Tiedemann and Treviranus' celebrated Physiological Journal, appear at greater intervals than every two or three months, while the greater number appear monthly, it is evident, *considering each number of a journal as a separate publication*, that the periodicals actually out-number all the other medical publications. This is a curious fact, and at first seems to imply, that the German writers have abandoned their old predilection for deep learning, and, consequently, have preferred making journals, rather than books published at their own risk, the medium of their communication with the public. An attentive examination of the German periodicals will, however, convince any one, that the contributors to these works are quite as remarkable as their countrymen have at any time been, for the extent of learning and originality of research which their writings display. In truth, for several years, some of the most important accessions our science has received, have been communicated in the German Journals.

The following list contains the titles of those which seem best adapted for the purposes of the British student.

1. *Archiv für Anatomie und Physiologie, von F. Meckel.*

Published every second month. Four parts make a volume. Each part costs one dollar.* This Journal has been continued in its present form since 1826, and was in fact a new series of the *Deutsche Archiv für Physiologie*, which commenced in 1815. I am not aware whether it is to be continued since the late and lamented death of Meckel; but it probably is, as its reputation is deservedly great, and its contributors numerous. The volumes already published form a standard work, and are indispensable to the physiologist, as they contain a great number of the original essays in which most important discoveries in comparative anatomy and physiology were announced.

2. *Zeitschrift für Physiologie; in Verbindung mit mehreren Gelehrten, herausgegeben, von Friedrich Tiedemann, Gottfried Reinhold Treviranus, und Ludolph Christian Treviranus.*

This physiological Journal commenced in 1824, since which only four volumes have been published, containing two parts each. It does not appear at regular intervals. It is in quarto, and what is rare for German books, the paper and printing are excellent; it abounds in plates well executed, and the essays it contains are all executed by the ablest authors. Every number contains several important communications, and in fact, *the index of each volume is a register of discoveries*. Thus in the last number we have a masterly paper on the lymphatics of the umbilical cord and placenta, by Fohman, with an accurate representation of the lymphatics of these parts minutely injected.† It is curious enough how slowly the discoveries announced in Meckel's or Tiedemann's Journal find their way into our English books. The average rate of passage of a fact from Germany to England is about two years, and many seem to have lost their way altogether, for they have not yet arrived. In this respect, with all due deference to our countrymen be it spoken, the Americans have long been our superiors, and are much more speedily and more extensively supplied with German intelligence than ourselves. My late excellent and learned friend, Dr. Duncan, did much to abate this evil, and it must be confessed, that our literature is chiefly indebted to the journals of Scotland, medical and philosophical, for a knowledge of the progress of science in Germany. Lately, but only lately, our

* The dollar here mentioned is equal to three and two pence British.

† For an extract from this paper see the Scientific Intelligence.

English brethren have followed the example, so that ere long we may hope to see this department of our literature cultivated as it deserves. Tiedemann's Journal is dear, the price of each part being three dollars.

3. *Journal der praktischen Heilkunde, herausgegeben, von C. W. Hufeland und E. Osann.*

The character of this Journal is too well known to require any comment. It has long been more copiously quoted by English writers, than any other German publication, a circumstance probably owing not merely to the intrinsic value of the book, but also to the well merited reputation of Hufeland; this is a purely medical Journal, and consists altogether of original papers. It is published every month. Six parts make a volume; the yearly subscription amounts to five dollars, and two thirds of a dollar.* The series commenced in 1795.

4. *Wissenschaftliche Annalen der gesammten Heilkunde, herausgegeben, von Dr. Julius Friedrich Carl Hecker.*

This Journal has fifty contributors, all men of eminence, and several of them foreigners. It consists not merely of original communications but of reviews, and I particularly recommend it to the English student. It embraces the various departments of medicine, surgery, and physiology; any one who can afford to take but one German Journal, would consequently do well to choose this. A part appears every month. The name before 1833, was *Litterarische Annalen*, and the annual subscription amounts to eight dollars. This Journal commenced in 1825. The back numbers may be purchased at a small price, viz., the first twenty-four volumes, from 1825 to 1832, for twenty-one dollars, and one third of a dollar.

5. *Archiv für medizinische Erfahrung, von C. Horn, F. Nasse und W. Wagner.*

This Journal, also published in Berlin, enjoys an extensive circulation; it is published every second month, and costs one dollar a number; it commenced in 1801.

6. *Heidelberger Klinische Annalen, von M. J. Chelius, C. F. Harless, &c.*

This appears, I believe, quarterly, and I am told by some

* To prevent confusion, I have thought it better to make use of fractions of a dollar, than of the smaller coin called a groschen.

German friends, is an extremely good publication. It costs one dollar a number, and includes surgery as well as medicine; it began in 1825, and in the course of a few years, a coalition was formed between its editors, and the conductor of a Journal called *Jahrbücher der deutschen Medizin und Chirurgie*; which latter was thenceforth embodied in the former, after having continued from the year 1819.

7. *Medicinische Zeitung herausgegeben, von dem Verein für Heilkunde in Preussen.*

This Journal is especially worthy of attention. I know none more valuable. It appears in folio form, and contains four or six pages. It is published weekly, and is edited by Dr. Hecker, the author of the well known history of the Black Death in the fourteenth century. It is very cheap, the subscription for the whole year amounting to three dollars, and two thirds of a dollar. It is dedicated to surgery as well as medicine, and contains notices of the chief works published in Germany.

8. *Journal der Chirurgie und Augenheilkunde, von C. F. Von Gräfe, und P. H. Von Walther.*

This has long enjoyed a considerable reputation in Britain, where late occurrences have contributed to spread the surgical and ophthalmic fame of Gräfe. It is published every second month, and commenced in 1820. Each number costs one dollar.

9. *Journal für die Ophthalmologie, von F. A. Von Ammon.*

This is a Journal devoted exclusively to ophthalmic surgery, but which I have never seen. Two volumes, each consisting of four parts, and costing three dollars, appeared in the course of three years.

10. *Journal für Geburtshülfe, Frauenzimmer, und Kinderkrankheiten, von E. C. J. Von Siebold.*

This enjoys, and most deservedly, a very high reputation, and is, as its title implies, exclusively devoted to midwifery, and the diseases of women and children. It was commenced in 1809, by Siebold, the father, under the title of *Lucina*, which was changed to the present title in 1827. After the elder Siebold's death, the work was continued by his son, formerly Professor at Marburg, but now at Göttingen. Each number costs one dollar. It is published at long intervals.

As we have in Britain no periodicals dedicated exclusively

to midwifery, I have thought it well to add the titles of two others published in Germany.

Gemeinsame deutsche Zeitschrift für Geburtskunde, herausgeben, von W. H. Busch, L. Mende, und F. A. Rittgen.

A volume consists of four numbers, and costs one dollar and a half. The numbers appear quarterly.

Annalen der Klinischen Anstalten der Universität zu Berlin für Geburtshülfe, Krankheiten der Weiber und Kinder, von Professor Betschler.

ROBERT J. GRAVES.

SCIENTIFIC INTELLIGENCE.

CHEMICAL AND PHYSICAL SCIENCE.

Spark during the Freezing of Water by Æther.—M. Julia Fontenelle states that M. Pontus, Professor at the Royal College of Cahors, has communicated to him the following observation. It is well known to chemists that if a phial, terminated by a small tube one to two centimetres long, be filled with water as well as the tube, and surrounded with cotton moistened with æther, the water freezes during the evaporation of the æther under the receiver of the air-pump. On repeating this experiment, M. Pontus remarked, that some moments before the congelation occurs, a spark, visible in daylight, escapes from the small tube which terminates the phial. This phænomenon is so generally true, that every time that he perceived the spark, he concluded the congelation was about to take place; and, on the contrary, when he did not see it, he presumed that the congelation was not near. M. Pontus was never disappointed in his conclusions. M. Fontenelle states that he also has seen the spark, and that M. Becquerel had remarked to him a similar effect at the moment of the formation of crystals in solutions.—*Lond. and Ed. Philosoph. Mag. and Jour. of Science.*

Test for Hydrocyanic or Prussic Acid, and Method of appreciating the Quantity.—We are informed by Mr. John T. Barry that the nitrate of silver, in common with other salts of that metal, is so extremely delicate a test of the presence of hydrocyanic acid, that its detection is not difficult in a drop of water containing far less than *the ten thousandth part of a grain* of that poisonous agent. For instance, if one minim of the dilute medicinal solution be mixed with a pint of water, its presence may be demonstrated in a single drop of the mixture. But what is of more consequence is, that although the mixture be contaminated with various organic substances, such as those contained in articles of diet, milk, coffee, tea, porter, wine, and soups, so far as is yet known the test retains its sensibility unimpaired. Mr. Barry, however, thinks that this extreme sensibility, while it renders the evidence of the silver test conclusive as to the *absence* of prussic acid, will be of more limited service in establishing its *presence*, for, without adverting to the possibility of other volatile substances being hereafter discovered to have a similar effect

on solution of silver, it is to be borne in mind that this reagent indicates the existence of prussic acid in some esculent substances where it had previously been found, as well as in some new ones. Upon this branch of the subject medical jurists will probably think it right to collect information.

The application of the solution of silver is simple. The suspected fluid is to be acidulated by the addition of acetic acid, but so as to redden litmus paper in only the *slightest degree*. If excess of acid be already present, it is to be *not quite* neutralized by carbonate of soda. These precautions are adopted to retard the interference of ammonia or muriatic acid. Two or three drops, quite cold, are then put into a watch glass, and immediately covered by a plate of glass, whose under-surface, to the breadth of a pea, is moistened with solution of nitrate of silver, formed by dissolving one grain lunar caustic in 100 grains distilled water:—

If the inverted drop of silver solution retain its transparency unaltered, the *absence* of prussic acid is established; for had it been present, the silver solution would in a few moments have become clouded by the formation of a *white* precipitate, an effect which, indeed, is almost instantaneous when the prussic acid is not excessively diluted. If, on the other hand, the precipitate appear, the conclusion must not be drawn that it is *cyanuret* of silver, until identified as such by two properties:—first, its speedy *re-solubility*, as denoted by the clouded drop becoming again clear, when *placed* over a vessel of caustic ammonia, in which respect it differs from the silver compounds of iodine and bromine:—and secondly, in retaining *unchanged* its pure white colour after exposure a few minutes to the sun's rays, or for a longer time, to day-light. As this property essentially distinguishes it from the compound of silver with chlorine, it is important to establish it by a separate experiment upon a somewhat larger portion of the precipitate, which should be obtained, by candle-light, by successively placing the inverted drop of nitrate of silver over renewed portions of the liquid in a saucer: as soon as the precipitate separates into distinct curd-like particles, it is ready for exposure to the solar rays.

Another property which distinguishes the cyanide (or cyanuret) of silver from the chloride, is, that upon being ignited in an open short glass tube, the cyanogen burns with a flame of the usual colour, leaving the metal pure, if sufficiently heated,—a quality the more valuable as it furnishes an index to the *proportion* of prussic acid it represents, which upon ordinary occasions may be estimated as equal to one fourth the weight of residual silver.

When, acting upon this principle, it is desirable to ascertain the *entire* quantity of prussic acid, it is to be obtained by slowly distilling over, in nearly filled close vessels, about an eighth of the acidulated mixture under examination; rectifying it; re-acidulating by acetic acid; precipitating by *slight* excess of nitrate silver; washing with distilled water, only so long as the washings affect

litmus paper; drying at 212° ; weighing:—and lastly, igniting and re-weighing.

The medicinal solution above referred to (as to be diluted for experiment in the proportion of one drop to the pint of water) contains, in round numbers, nearly a sixteenth of its own weight of anhydrous prussic acid, or rather less than four grains in the drachm, being the article (commonly designated “of Scheele’s strength,”) as manufactured by some respectable houses in London. We understand that Messrs. William Allen and Co., by means of silver as a reagent, have uniformly concentrated it to this degree since the year 1820, when Mr. Barry introduced the use of that metal to determine and regulate its proportion of absolute prussic acid by the formation of cyanuret of silver. The method being one which admits of extreme precision, will deserve the attention of the College of Physicians, if prussic acid be inserted in the next Pharmacopœia. It is to be recollected that this preparation, like those of alcohol or æther, is subject to variation, notwithstanding any superiority of formula, or care on the part of the operator. Hence, the necessity of means for assaying the final product and for reducing it to a uniform standard. With regard to the employment of cyanuret of potassium for the occasional formation of hydrocyanic acid, it is a question which at least deserves very serious consideration. Its disposition to absorb atmospheric moisture, and always to become more or less converted into carbonate, while its cyanogen (united to hydrogen,) to an uncertain extent is dissipated, especially when this beautiful salt is much disintegrated, constitute formidable difficulties. But a still greater objection will present itself at the counters of apothecaries and chemists where medicines are made up, from the possibility of this intensely poisonous salt being sometimes mistaken for other substances, in the frequent extemporaneous production of prussic acid.—*Ibid.*

Extract of a Letter from Mr. William Gregory to M. Robiquet.—“I have just examined the muriate of morphine, prepared after our mode, by M. Duncan. That salt is of a bright whiteness, and does not retain the least trace of narcotine. I operated on two kilogrammes after your method for the codéine, and from that quantity I obtained two ounces of the hydrated codéine perfectly soluble in æther and in water. I found in it all the properties so well developed in your last work; having thus at my disposal a considerable quantity of that new substance, I thought it right to make some experiments on its therapeutic properties. I prepared the crystalized nitrate, some of which I took myself, and several of my students who wished to try the effect: a dose of three grains had no effect on either of us, but a stronger dose, of from four to six grains produced very remarkable symptoms; first, the pulse was accelerated, great heat in the head and face; then the spirits were remarkably excited as if by strong liquors, an agreeable excitement which lasted some time; it was accompanied with great itching, which commenced at the head and

spread all over the body; after a lapse of some hours, that state is followed by a disagreeable depression, with nausea, and sometimes vomiting. Neither of us felt any disposition to sleep, excepting after the depression. We did not try a larger dose, but it appears that the codéine possesses a stimulant effect, and produces violent itching in a dose of five grains. Such is the ordinary progression of symptoms which it produces, but in some its effects, especially in the state of depression, are very disagreeable.

“After these experiments it is not probable, as you have supposed, that the presence of the codéine is the cause of the superiority of the ordinary muriate of morphine. As one-fourth of a grain of that latter salt is sufficient for a dose, and only contains one-thirtieth of its weight of codéine, and of which three or four grains are required, the effect cannot be attributed to the codéine. It remains to find out if the muriate, deprived of codéine, is less stimulating on that account, and if it does not cause the same itching I have seen often produced by the non-purified muriate, and also by the opium, in a great number of cases: it is a point which I purpose examining hereafter.

“If you believe these remarks can interest the Society of Pharmacy, have the goodness to communicate them—perhaps some of the members will be curious enough to repeat them with the codéine which you have prepared. I should have added, that in two or three cases the codéine produced a slight purgative effect, while in others it was inert in that respect.”

I had also said that morphine was far from representing the essential properties of opium, and that they probably completed it: the observations of Mr. William Gregory confirm, in a great measure, my ideas. But it is remarkable that that able chemist only employed the codéine in the form of nitrate, and the experiments of M. Kunkel have shewn, that the codéine loses much of its action when it is combined with acids. It is then to be presumed that in employing that new alcaloid in simple aqueous solution, the effects will be much more marked than those observed by Mr. Gregory.—ROBIQUET.—*Journal de Pharmacie*, Feb. 1834.

On the Rotatory Movements of Camphor, by Mr. Charles Mattucci.—In studying the phenomena of rotation which small pieces of camphor presented on the surface of water, M. Mattucci has shewn, by direct experiment, that it is the current of the vapour produced by that volatile substance which causes the rotation, and not the development of electricity, as some persons have supposed. That physician has remarked, that in placing the vessel where the phenomenon of the rotation of a large piece of camphor was scarcely perceptible in the open air under the pneumatic machine, its movement became more rapid as soon as the air was rarefied, and that it stopped when they ceased to extract the air. He observed all these phenomena of rotation on the water in all solid volatile bodies, or those

impregnated with a volatile substance. For example, in placing cork filings impregnated with sulphuric æther on the water, they will turn very rapidly like small pieces of camphor; and if it is wished to continue that rotation for some time, one end of a piece of thread may be dipped in æther, and the other end brought in contact with the surface of the water: in this manner the æther will descend as by a syphon, and the movement will be prolonged.—*Journal de Chem. Med.* Feb. 1834.

NATURAL HISTORY.

Extract from a Paper read by Dr. Houston, at a Meeting of the Zoological Society of Dublin, on the Diseases of the Animals which died in their Collection.—Among the animals of the class *Mammalia*, the causes of whose death I have ascertained by dissection, are several of the monkey tribe, a Sambar deer, and a Virginia deer.

Of birds I have made some interesting observations on the diseases which have deprived us of an eagle, an ostrich, and a macaw.

Respecting the monkeys, I have had opportunities of examining the bodies of a great number of this tribe of animals, and I cannot charge my memory with a single case, in which the animal had not either died of consumption, or, having lost its life from some other cause, did not present marks of incipient phthisis in its lungs, and generally evidences of the same scrofulous disease exhibited themselves simultaneously in many other organs of the body, such as the liver, spleen, lymphatic glands, kidneys, uterus, brain, &c.

In the little animal which every one must have seen in the streets, riding as a soldier on the back of a dog, and which died lately, leaving its proprietor penniless, one of the lungs was completely gone, and the other far advanced in disease. In some parts, small recent tubercles were found.—(Here Mr. Houston explained the nature of tubercles.)—And in others tubercles of longer standing existed, as evidenced by their size, and conversion into a white cheesy matter. One of the lungs was also universally adherent to the inside of the chest, and motionless. Rudiments of the same disease had commenced in the liver and spleen.

In the animal which last died in the gardens, the Barbary ape, the lungs were filled with small tubercles, not, however, in so advanced a state as in the animal last alluded to. The lymphatic glands behind the roots of the lungs and heart, were very much enlarged; their structure totally changed, and their interior occupied by masses of scrofulous matter. By their size and number, they surrounded, and inconvenienced the operations of the great blood-vessels and œsophagus or gullet. The liver was studded, throughout, with numerous incipient points of the disease; the spleen was so changed by it, as to have been rendered incompetent to the performance of

its natural functions. The glands of the mesentery were enlarged, constituting the disease, which in children is called *tabes mesenterica*. The structure of the uterus was every where occupied by round, soft, and white tumors; and one of the kidneys had taken on a similar diseased action. I have preserved in the Museum of the College of Surgeons, a specimen, showing extensive disease of the same kind in one of the hemispheres of the brain of a monkey; and I have noticed, in another case, ulcers on the lining membrane of the intestines, in connexion with a tuberculated condition of the glands about those organs. In contrasting the scrofulous disease acquired by these animals while resident in our climate, with that which affects human beings, (for the disease in both is exactly the same,) my observations would tend to the conclusion, that, in the former, the scrofulous taint spreads itself more generally over many tissues and organs, destroying the animal rather by its universality than by concentrating itself, and causing great alterations of structure in any one organ; whilst in the latter, it confines itself more usually to one organ, the lungs, and causes death by the production of such changes therein, that their all important functions are annihilated. In none of the monkeys dissected by me, had the disease of the lungs reached beyond what are considered the first and second stages of it in the human body. In none were the tubercular deposits in any other form than that of small, circumscribed, soft tumors, without the presence of much purulent matter for expectoration; the animal appearing to have died from the general febrile state induced by lurking disease in so many parts of the body, rather than from privation of air, on account of the incapacity of the lungs to receive it; whereas it is, to medical men, a matter of every-day observation, to find the lungs of persons dying of consumption, converted into great cavities, which discharged, during life, enormous quantities of matter, and by occupying the place of sound lungs, almost deprived the individual of breath.

We may now allude to the post mortem examination of the Samber deer, the death of which was a source of much regret at the garden. The animal had been a considerable time ailing—it had eaten badly, and lost flesh—but further than this no suspicions were entertained of its being dangerously ill, until a few days before its death, when it contracted a cough, appeared weak, and much disposed to lie on its litter, while at the same time its restlessness indicated the presence of pain. On examination, the cause of death was found to be in the lungs, which exhibited marks both of the recent disease of which the animal died, and traces of the former alterations in structure, on which its previous delicacy depended. The latter were exhibited in a solidified, tuberculated state of the lower part of the lungs on both sides. The former were evinced by engorgement in the right lung, and effusion of lymph in the pleura. The animal had had disease of both lungs of long standing, and was killed by a recent attack of pleuro-pneumony of the right. No other disease was found in any part of the body except a caries of the lower jaw on the right side, near the articulation, an affection, the presence of which may account for the

difficulty of chewing, which had been observed in the animal for a considerable time previously to death. The heart, the intestines, and all the other organs appeared in a sound state. We should learn from the facts here stated, to pay particular attention to animals which happen to be in any way indisposed, because, as in the human constitution, the more delicate the frame, the more susceptible is it of inflammatory attacks, and the more care is necessary for its preservation. Indeed, so far as I can recollect, the death of the Samber deer occurred in the beginning of last winter; when, with lungs in a previously delicate state, and highly susceptible of aggravation of the disease under which they laboured, from causes which in the natural state would not be felt by them; most probably some cold, accidentally taken, may have been the cause of the recent malady.

The Virginia deer, the beautiful little animal which used to attract so much attention by its familiarity, in jumping about the visitors, and taking bread off the hand which presented it, met its death by being gored by the hog-deer. In this animal no traces of disease could be found in any organ in the body. The liberty which was allowed it on account of its harmlessness, secured for it natural exercise, and might have preserved it alive for a long period, had not the misfortune before mentioned occurred to it.

A golden eagle, in a state of apparent good health, became suddenly ill, and died after a lapse of a few hours, as if in a fit of suffocation. No disease was found in any part of the body, except the windpipe, which gave evidences of the most rapid and violent inflammation. The interior, near the lower larynx was red, and covered with a coating of lymph, which adhered loosely to the lining membrane; and was in such abundance as nearly to block up the passage and obstruct the entrance of air. The disease was like croup in children, called the pip. The lower part of the larynx in birds, Mr. Houston observed, is the part where the sound is produced—in man, it is the upper part: the bird was affected in the lower part of the larynx, children are affected in the upper part. The next case is, that of a young Solan goose, (*Anser Bassanus*), which died of a very singular affection. It had pined away during weeks; refused food, and died. The windpipe was sound. The air cells all through the body were in a state of chronic inflammation thickened; coated with lymph; white and moist in some places; in others dry, green, and blue, moulded from the air. The cells around the heart and great vessels, in arm bones, about the liver, and all through the abdomen, were in this state. The texture of the lungs was otherwise sound; there existed no tubercles.

The female ostrich received the stroke of a branch in the left eye. The eye swelled greatly, was protruded from the socket, red, and discharging matter. The animal was pining away; its limbs were wasted and pale; its feathers dropped out, and it could not be got to eat. Under the impression that a collection of matter had formed in the interior of the eye, and was prevented from getting out by the thick tissues, (and satisfied that the sight of the eye was

for ever lost,) I made an incision through and across the cornea with a lancet, and gave exit to the pent up discharge, which, by the pain and fever it produced, was acting so deleteriously on the health of the bird. After this the eye sunk back into its socket, though retaining its natural fulness. It gradually healed over; and the animal, though deprived of sight in that eye, is now much improved, and promises soon to be restored to health.

The death of the macaw was caused by a round, firm, whitish tumor, which grew in the coats of the intestine, about a foot and a half from the cloaca, and obstructed the passage through the bowels; the part of the gut above the tumor was distended with alimentary matter in a semifluid state, and appeared thick and large; that below, which naturally is the widest part, was small, attenuated, and empty. Similar diseased tumors were found in the liver and spleen. The skeleton also presented a specimen of the reunion of broken bones; one thigh and tibia having been fractured at some former period, and strongly, though not very elegantly, repaired by irregular masses of callus.

But the most interesting part of the examination of this bird was connected with the stomach.

The following is a description of the *œsophagus*, stomach, and intestines of a macaw (*macaceros macao*). The *œsophagus* is short and wide; the crop large, thin, dilatable, and vascular; the *ventriculus succenturiatus* is thick and glandular, and large and stomach-like where it joins the gizzard; the gizzard is not bigger than a good sized hazel-nut, and has more the appearance of a knobby appendix to the *infundibulum*, than a part of the organ from which much assistance could be derived in the process of digestion. Most probably in this bird, which was very old, and had been a long time domesticated and fed chiefly on pap, the condition of the stomach had been changed from that which in the natural state adapts it for bruising the husks and shells of fruits, to that more suitable for the chymification of soft and digestible materials. The intestines are long and without *cæca*. This singular transformation of an organ into one of a different type, on account of a change in its application to the purposes of digestion, is peculiarly interesting in a physiological point of view. We have here, without design, without any pains-taking on our part, and without any cruelty to the animal in question, an illustration of a fact respecting the stomach, for the establishment of which a great variety of experiments were instituted. It is a transformation exactly analogous to that effected by the celebrated physiologist, Duhamel, in the stomachs of pigeons, by feeding them solely on animal matters. The gizzard of the granivorous bird (the pigeon) underwent a species of metamorphosis—it was converted into the membranous stomach, peculiar to the bird whose natural aliment is flesh.

ANATOMY AND PHYSIOLOGY.

*Analysis of a Memoir on the Mechanism by which the Sounds of the Heart are produced, read to the Academy of Sciences of Paris, 3rd of February, 1834, by M. Magendie. (First part).—*Laennec is the first who paid special attention to the study of the particular sounds of the heart, in the discharge of its function of keeping up the circulation. To him we are indebted for knowing, that during the natural performance of this function, the heart causes to be heard at each of its pulsations two successive, well-marked sounds, called, by reason of their particular physical characters, the *dull sound* and *clear sound*. But neither the mode in which the production of these sounds is explained by him, nor the explanations given of it since his time, are, or ought to be admitted. According to Laennec, the dull sound is produced by the contraction of the ventricles at the instant that the blood is sent into the aorta and pulmonary artery, the clear sound depending on the contraction of the auricles: the source of both the one and the other to be referred to the sonorous vibrations, developed in the muscular fibres of the heart, at the instant when they contract.

But to this explanation it may in general be objected, 1st, that it is founded on deductions drawn from the time and place where each of these sounds peculiar to the heart are more especially produced, and not on direct observations; 2nd, that the production of the sound by the contraction of the muscular fibres of the heart, is a supposition quite gratuitous; 3rd, that even admitting it, we cannot understand how the ventricles of the heart, distinct in their situation, and different in their structure, should move constantly with such harmony, as never to produce but one single sound; 4th, neither can we understand, how in certain cases, these sounds cease altogether, the heart still continuing to act and to keep up the circulation; whence it would follow, that there are sonorous contractions, and others that are not so, &c.

Professor Turner, in a paper inserted in the third volume of the Transactions of the Medico-Chirurgical Society of Edinburgh, has particularly objected, that the clear sound cannot be attributed to the cause assigned by Laennec, seeing that the venous pulse of the jugulars, which depends in a great measure on the contraction of the auricles, presents no coincidence with this sound, which, however, should happen, if they had the same origin. But he does not himself offer any explanation of the phenomenon, declaring that he knows none.

In 1830 and 1831, Doctor Hope, relying on numerous experiments, still further objected; 1st, that the contraction of the auricles precedes the dull sound of the heart; 2nd, that this contraction is not accompanied by any sound; 3rd, that the clear sound of the heart coincides with the dilatation of its ventricles. And substituting ano-

ther explanation for that of Laennec, he refers the two sounds of the heart to the sudden displacement which the blood undergoes, either in entering the ventricles, or in its exit from them. But this explanation, devoid of proofs, is itself exposed to the strongest objections. Likewise since Dr. Hope's work, other explanations have been proposed. Sometimes the sounds have been attributed to the shock of the blood against the parietes of the ventricles at the moment of their dilatation. Sometimes they have been made to depend on the shaking of the aorta and pulmonary artery, under the influence of the blood driven forwards by the ventricles. More recently M. Ronanet, in his inaugural dissertation to the faculty of Paris, endeavoured to prove by a series of arguments and some experiments, that these sounds are owing, the one to the shock given by the blood on the tricuspid and mitral valves, the other to the shock of the blood on the sigmoid valves of the aorta and pulmonary artery.

In this question the first point, as we see, is to examine whether the sounds of the heart are developed in the organ itself, as was supposed by all the physicians who endeavoured to discover their origin, or whether they have any other cause. "It is with this view," says M. Magendie, "that I first instituted the following experiments :

"*First experiment.* I listened to the heart, when exposed in a living animal, still keeping up by its alternate movements the circulation of the blood. The experiment first made, and several times repeated on small animals, was afterwards made on larger animals, dogs, horses, and every time the heart was exposed and insulated, the sound entirely ceased.

"*Second experiment.* As mammiferous animals survive only a short time after removing the anterior part of the thorax, and the celerity with which the observation should be made, may give rise to error, I wished to repeat the experiments on birds, where the sounds of the heart are well developed, and in whom it is possible entirely to remove the sternum without immediately destroying life. When instituted on geese, the experiment yielded me the same results.

"Thus, I at first attained the certainty, that the sounds of the heart are not developed in the organ itself, that is to say, that they do not arise from the respective play of its different cavities, no more than from the action of the heart on the blood, or of the blood on the heart."

This point settled, the question is to discover the true origin of these phenomena.

"*Third experiment.* I had the idea," continues M. Magendie, "that the sounds of the heart might be produced by the well known beating of this organ against the parietes of the chest, and I saw no physical reason which could make me reject this idea. On the contrary, I could not understand how a shock on an elastic parietes, such as the thorax is, should not be sonorous. To verify my conjecture, having laid bare the heart in several living animals, I exposed sonorous bodies to its impulse during the systole of the ventricles. Every

time these shocks produced distinct, and sometimes very intense sounds, which varied also according to the nature of the body that was struck.

“ *Fourth experiment.* I raised with the greatest care the sternum of a goose; immediately the sounds of the heart ceased, as I already stated: then I replaced the bone in its natural position; and immediately the sounds recommenced, such as they were before the experiment. By raising and replacing the bone, I could easily suspend and produce the sounds.

“ With these experiments the following facts are connected:

“ 1st. In certain animals whose chest is spacious, and the membranous attachments of the heart flexible, as happens in that species of dog called the *Spanish pointer*, it is sufficient to make them lie on the back, and thereby separate the heart from the thoracic parietes, when the sound ceases forthwith, recommencing again when the animal resumes his ordinary position.

“ 2nd. Air or water introduced into the chest by pushing back the heart towards the vertebral column gave the same results, so that the animal subjected to this experiment presents no sounds of the heart, though that organ contracted regularly.

“ 3rd. Even in man when there exists any mechanical obstacle interposed between the heart and the thorax, the sound of the heart ceases in the same way.

“ Thus I acquired in the second place a certainty, that the sounds of the heart were produced, conformably to what I had conjectured, by the shocks of this organ against the parietes of the chest.”

This point being settled, it still remains to find the cause of the difference of the two sounds.

“ *Fifth experiment.* I introduced a slip of metal, thin and flat, into the thorax of a dog, so as to prevent the shock of the apex of the heart against the thoracic parietes; immediately the dull sound ceased. The efforts of the heart on the metal plate were perceptibly violent, and if I ceased for a moment to press on the heart, the sound recommenced, the apex of the organ striking against the parietes of the thorax.

“ Air or water injected into the left pleura, so as to keep the heart at a distance from the wall of the thorax, yielded me a similar result.

“ *Sixth experiment.* The same metal plate being laid on the anterior surface of the right ventricle, so as to keep it at a certain distance from the sternum, the clear sound ceased immediately, to commence again the moment the plate was withdrawn. Thus I again attained the certainty, that the dull sound of the heart is evidently the result of the shock of the apex of this organ against the chest; a shock which is designated for time immemorial, by the excellent name of *pulsation of the heart*, (*battement du cœur*), and that the clear sound is caused by the shock of the anterior surface of the right ventricle against the posterior surface of the sternum, and the neigh-

bouring parts of the thorax on the right side, at the moment of the dilatation of the ventricles.

“ With respect to this double shock of the heart on the thorax, the one at the instant of the contraction of the ventricles, the other at the moment of their dilatation, it was proved to me by a considerable number of direct observations made on living animals. It is also easy to verify it in man, in certain cases of hypertrophy of the ventricles; for if two stethoscopes be placed simultaneously on the chest, the one opposite the apex of the organ, the other on the lower and left part of the sternum, with a little attention we distinctly perceive the two instruments violently raised at a very short interval, and perfectly isochronous with each of the sounds of the heart.

“ To sum up, here then, if I am not mistaken, is the true mechanism of the normal sounds of the heart :

“ In contracting, and for causes long since known, the ventricles throw the apex of the heart against the left lateral part of the thorax, and thus produce the first sound, i. e. *the dull sound*.

“ In dilating in a great measure under the influence of the rapid (*brusque*) influx of the blood, the heart gives a shock to the anterior parietes on the right of the thorax, and thus produced the second sound, *the clear sound*.

“ This explanation, conformable to the general laws of acoustics, and which will henceforward, I trust, form a new point of contact between medicine and physics, is so simple and so natural, that one might ask how it did not come into the mind of physiologists at first, did we not know that in the sciences, as in other matters, simplicity and truth very rarely present themselves in the first instance to our imagination.”

But because, if this explanation is the true one, it ought to account for all the particular circumstances connected with each of the sounds of the heart, M. Magendie will examine this question in the second part of his memoir.—*L'Institut. Journal General des Societes*, &c. Feb. 1834.

Discovery of Lymphatics in the Human Placenta and Funis, by Dr. V. Fohmann.—It is well known that the placenta and funis rank foremost among those parts in which the presence and distribution of absorbents have not yet been duly recognized. Some anatomists, certainly, such as Everhard, Pascoli, Needham, and Röslin, who are cited by Schreger,* as also Wrisberg,† and Uttini,‡ insist

* De Functione Placentæ Uterinæ, ad virum illustrem S. T. Sömmering Epistola. Erlangæ, 1799.

† Michaelis Observationes Circa Placentæ ac Funiculi Umbilicalis vasa Absorbentia. Göttingæ, 1790.

‡ Ueber die Einsaugenden Gefässe des Mutterkuchens; in Meckel's Archiv. vol. ii.

that they have seen absorbent vessels in these parts ; but little reliance was placed on their observations, because men of great reputation, such as Hunter, Hewson, Cruikshank, Mascagni, and others, had in vain endeavoured to demonstrate them. Moreover, as, after the establishment of Hunter's doctrine of absorption by the lymphatic vessels, new experiments were made which appeared to favour the theory of absorption by the veins, and the attempts to discover absorbents in the placenta and funis were unsuccessful, these parts were, without farther consideration, declared to be unprovided with absorbents, and were even employed as a proof of absorption by the veins. It was maintained that venous absorption was not only proved by numerous experiments on live animals, but also by those parts which, though not provided with absorbents, were yet organs, such as the placenta, eye, &c. in which the existence of an active absorption could not be denied. Now, in this, there was evidently a false conclusion. The fact that absorbents have not been found in certain organs does not prove their non-existence in those organs, but only that we have not been able to discover them. In general, physiologists relied too much on superficial examination of these vessels, and were thereby led into many errors. Thus Magendie contested Monro's and Hewson's doctrine of the presence of absorbents in all the *vertebrata* ; and still denies the existence of vasa chyliifera in birds. Fishes and amphibia have a perfect absorbent system; the intestinal canal of birds, and many other parts said to be without absorbents are plentifully provided with them ; and the placenta and funis are, as we shall prove, in the same predicament.

The funis consists, its blood-vessels excepted, of a tissue of absorbents, which is so close that the point of a needle cannot be introduced into any part of it without wounding one. Nothing can be easier than to inject them with mercury. Had the anatomists who asserted that they observed absorbents in the funis, been rightly aware of their situation, it would have been easy for them to convince their opponents of the truth of their statement by ocular demonstration. To fill them with mercury, nothing more is necessary than to pierce the sheath with a small lancet, and then inject the metal through a fine tube. As a great many vessels are necessarily wounded in this operation, and the mercury, on entering one branch, gets into others through numerous anastomoses, it is evident that the experiment must easily succeed. It must here be remarked that the filling of the absorbents of the funis is the more readily accomplished, the fuller it is of the *liquor Whartoni* ; which no doubt arises from the fact that that fluid distends the vessels (absorbents) in which it is contained, and consequently favours the entrance and advance of the mercury. For the rest, it would be vain to endeavour to discover in the funis and placenta absorbents provided with valves, as Wrisberg pretended he saw, or as we find them after leaving the venous organs of the body. The absorbents of these temporary structures do not attain to such a degree of development, but are circumstanced as they are in the parenchyma of the organs : that is to say, they form

tissues and net-works, of which the branches are valveless, so that mercury injected into any one can proceed in all directions.

If we wish to enter minutely into the examination of the absorbents of those parts, by means of which the communication between the mother and foetus is maintained, we have to consider those vessels as they exist in the placenta and funis, as well as their passage into the body of the foetus, and their distribution therein. The funis, its blood-vessels excepted, consists solely of a tissue of absorbents, as has been already mentioned. The larger ones occupy its axis, while the smaller ramify towards the surface, and are at last lost in the sheath. The cells or vesicles usually observed in the funis, are shown by injection to be dilatations of the absorbents, into which branches from the placental side open themselves, and continue their course to the foetus on the opposite side. The absorbents of the funis become perceptibly finer at each of its extremities, and especially where they enter the cutis at the umbilicus. But before I proceed further with the description of them in that situation, I must relate what I have observed of them in the placenta.

By injecting with mercury the tissue of absorbents at the placental extremity of the funis, and driving the metal towards the placenta with the handle of a scalpel, we may sometimes succeed in injecting a net-work of absorbents that is spread out between the placenta and its investing membrane. I never saw any vessels rising from this net-work and ramifying into the membrane, (the chorion,) as has been observed with respect to the sheath of the funis; and I but seldom discovered any branches penetrating the parenchyma of the placenta. Where they proceed when they do penetrate, I have not been able to make out; but I am inclined to think they reach its uterine surface.

On the passage tissue of absorbents of the funis into the abdominal region of the foetus, the superficial ramifications at the distance of a few lines from the umbilical ring, become so very minute as to be scarcely discoverable by a strong lens, even when injected with mercury. On the other hand, the deeper seated vessels gain in strength while they lose in diameter; so that we can employ the handle of the scalpel to drive the mercury on through them without any fear of their rupture. On reaching the umbilical ring, they become somewhat larger; and some of them run into the dense tissue of absorbents between the epidermis and cutis, of which, the sheath of the funis is only a continuation. The rest unite into branches, which proceed under the cutis, and at the distance of some lines from the umbilical ring generate a lymphatic trunk, which running in a circular direction, forms another ring. This gives off branches which, following the external abdominal veins, descend between the cutis and the muscles to the inguinal region, and pass under the crural arch to the iliac glands. I have invariably observed this distribution; but I have sometimes also found lymphatic branches beneath the abdominal muscles, proceeding from the funis through the

umbilical ring, and following the vein or arteries into the porta of the liver, or else running to the glands which receive the vessels that descend without the abdominal muscles.—*W. West.*

*Observations on the supposed Vision of the Blood-vessels of the Eye, by Sir David Brewster, LL.D., F.R.S.**—In the Number of this Journal for September, 1832, I had occasion to refer to the remarkable experiment described by Dr. Purkinje of Breslau, in which the blood-vessels of the retina are supposed to be exhibited; and though I had in vain tried to see this phænomenon, yet it had been so accurately described to me by Mr. Potter that I ventured to give an opinion respecting its cause. The paper which contained this explanation was read at the Physical section of the British Association at Oxford, in June, 1832, and Mr. Wheatstone, who was present, favoured the Meeting with some excellent observations on the subject. These observations have been printed in the Report of the Association, for 1832, in the form of an Appendix to the abstract of my paper; and as they are highly interesting, and will form the groundwork of the following observations, I shall give them *verbatim*.

“After the reading of Sir David Brewster’s paper, Mr. Wheatstone said, that having been the first person to introduce Purkinje’s beautiful experiment into this country, and having repeated it a great number of times under a variety of forms, he would take the opportunity of stating a few particulars respecting it, which appeared not to be generally known. The experiment succeeds best in a dark room, when, one eye being excluded from the light, the flame of a candle is placed by the side of the unshaded eye, but so as not to occupy any of the central part of the field of view. So long as the flame of the candle remains stationary, nothing further occurs than a diminution of the sensibility of the retina to light; but after the flame has been moved upwards and downwards, through a small space, for a length of time, varying with the susceptibility of the individual on whom the experiment is tried, the phænomenon presents itself. The blood-vessels of the retina, with all their ramifications, exactly as represented in the engravings of Scemmerring, are distinctly seen, apparently projected on a plane before the eye, and greatly magnified. The image continues only while the flame is in motion: directly, or soon after, the flame becomes stationary, it dissolves into fragments and disappears.

“Mr. Wheatstone dissented from the ingenious explanation of this appearance offered by Sir David Brewster, and also from that opinion stated to be the generally received one; and begged to repeat the solution he had published, and which he had not since been induced to relinquish. Mr. W. observed, that there was no difficulty in accounting for the image; it evidently was a shadow resulting from

* See Lond. and Edinb. Phil. Mag., last Number, p. 43, and also vol. i. p. 318.
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the obstruction of light by the blood-vessels spread over the retina; the real difficulty was to explain why this shadow is not always visible. To account for this Mr. W. adduced several facts, which tended to prove *that an object, either more or less luminous than the ground on which it is placed, when continuously presented to the same point of the retina, becomes invisible; and the rapidity of its disappearance is greater as the difference of luminous intensity between the object and the ground is less; but by continually shifting the place of the image of the object on the retina, or by making it act intermittently on the same point, the object may be rendered permanently visible.* To apply this explanation to the phenomenon in question, Mr. W. observed, that whenever the flame of the candle changes its place, the shadows of the vessels fall on different parts of the retina; which is evident from the motion of the figure while the eye remains still, which is always in a contrary direction to that of the flame. Hence the shadow, being thus made to change its place on the retina, remains, according to the law above stated, permanently visible; but instantly the flame is at rest, the shadow also becomes stationary, and consequently disappears.

“ Mr. Wheatstone then exhibited an instrument for showing an original variation of this experiment: it consisted of a circular plate of metal, about two inches in diameter, blackened at its outer side, and perforated at its centre with an aperture about as large as an ordinary gun-hole; to the inner face was fixed a similar plate of ground glass. On placing the aperture between the eye and the flame of a candle, and keeping the plate in motion, so as to displace continually the image of the aperture on the retina the blood-vessels will be seen distributed as before, but will now appear brighter, and the spaces between the ramifications will be seen filled with innumerable minute vessels, anastomosing with each other in every direction, which were invisible in the former experiment. In the very centre of the field of vision there is a small circular space, in which no traces of these vessels appear. Mr. W. remarked, that the absence of these minute obstructions to light will probably account for the greater distinctness with which small objects are there seen, and also for the difference of colour observed by anatomists in that spot of the retina.”

In this experiment Mr. Wheatstone has described, 1st, the common method of seeing the blood-vessels, and 2ndly, an original variation of the experiment by which the blood-vessels are seen much more distinctly and completely.

As Mr. Wheatstone was so kind as to lend me, when at Oxford, his plate of metal, &c., I was enabled to see the very phenomenon which he saw, and I have repeated the experiment fifty times since under many modifications. I have, therefore, no hesitation in asserting that the *ramifications* exhibited by Mr. Wheatstone's apparatus are not blood-vessels, but are nothing more than the ramifications described in my paper already referred to. (Lond. and Edinb. Phil. Mag., vol. i. p. 170. § 1.) If we throw aside the ground glass in

Mr. Wheatstone's apparatus, and look at a luminous surface through the circular aperture when moved as he describes, the same phenomenon will be seen: and if we substitute a rectilineal aperture, and make the line of motion perpendicular, or nearly so, to its longest sides, the phenomena will be seen still more distinctly; and if we look through one or more narrow slits, as in my experiment, the effect will be the same. In short, the edges of the circular aperture in Mr. Wheatstone's apparatus perpendicular to the line in which the aperture is moved, perform the part of the rectilineal slit or slits in my experiment. Mr. Wheatstone will have no difficulty in recognizing the perfect identity of the two experiments, and he will therefore see that the ramifications are nothing more than the new forms given to the luminous and dark parallel lines produced by the action of light upon the retina. In order to demonstrate this, let us use Mr. Wheatstone's own apparatus. The general direction of the ramifications is invariably perpendicular to the direction in which the aperture is moved. If we change this direction from a horizontal to a vertical line the ramifications change their direction also, so that we can give them any inclination we please. They cannot, therefore, be pictures or representations of any blood-vessels in the eye.

This unequivocal result would have induced me to believe that the ramifications seen by the common method had a similar origin, and were owing to the action of the rectilineal sides of the flame upon the retina, had I not succeeded in seeing this phenomenon with my own eyes. At the Observatory of Cambridge, last summer, Sir John Herschel pointed out to Mr. Airy and myself the method by which he saw the ramifications, and we were all successful in observing the same phenomenon. This method scarcely differed from that described by Mr. Wheatstone, but the ramifications which I saw were *toto cœlo* different from those produced by Mr. Wheatstone's apparatus: they had not, indeed, one property in common but that of *ramifying*. The one was seen with great difficulty and occasionally in the middle of a brownish red light, which *did not proceed directly from the candle*; while the other was distinctly and continuously seen in the middle of condensed light *proceeding directly* from the candle or other luminous body.

Regarding, therefore, the phenomenon as real, and the ramifications as occasioned by a blood-vessel of the retina, I shall proceed to examine the different explanations that have been given of it.

The explanation given by Mr. Wheatstone is exceedingly ingenious; and the principle which he lays down, and which is printed in *Italics*, is in every respect well founded. This property of the retina, by which it is unable to maintain the continued visibility of an object seen *obliquely*, or at a distance from the axis of vision, was communicated by me to the Royal Society of Edinburgh, on the 19th of January, 1818, and has since appeared in several elementary works on optics; and it is a necessary corollary from the law of oblique vision, that any movement of the object must restore its visi-

bility by removing the cause of its disappearance, namely, the continued action of the light upon the retina.

So far, therefore, Mr. Wheatstone's explanation is unimpeachable; but when he states that the motion of the flame causes *the shadows of the vessels to fall on different parts of the retina*, we can no longer follow him. Unless the blood-vessel is placed at a certain distance *in front of the retina*, and consequently in the vitreous humour, it can have no *moving* shadow; and unless it is within the refracted cone of rays which proceed from the candle, it can have no shadow either moveable or stationary. If the shadow here referred to, be the shadow produced from the direct light of the candle, then the blood-vessel would appear across the visible flame of the candle, and not at the side of it in the reddish brown light. But independent of these objections to the application of the optical principle previously laid down, there are two facts which appear to be conclusive against the explanation: the one is, that the blood-vessels of the retina are not at a distance from it; and the other, that the ramifications may be seen distinctly when the candle is not in motion.* There is one objection more to this explanation, which appears to me a formidable one: the ramifications ought to be distinctly and readily seen when the light which forms the shadow is reduced to the same state of dilution, and the same colour, as the reddish brown light on which they appear. This experiment I have repeatedly made with light of all degrees of dilution and divergency, but I have never been able to see a trace of the ramifications.

If the ramifications in question are the representation of a blood-vessel, it becomes very interesting to ascertain the cause to which their visibility is owing. The first step in the inquiry is obviously to determine the origin of the reddish brown light in which the phenomenon is seen. It is quite clear that the brown light is no part of the cone of refracted rays that proceed from the candle: it is equally clear that it is not produced by two or more reflections from the curved surfaces which bound any of the humours of the eye, because in this case it would be of the same colour with the light of the candle: and I have besides ascertained that it has no focus; for if it had, it would expand and contract by any variation in the distance

* The force of this last objection will depend on the circumstances of the case. Mr. Wheatstone says that the image "continues only while the flame is in motion," and that "directly, or soon after, the flame becomes stationary, it dissolves into fragments and disappears." Now if this is a phenomenon of oblique vision, the image ought not to disappear permanently. One part of it should disappear while another part remains visible, and the whole may for a short time continue invisible; but it will soon reappear, because it would require great steadiness, both in the hand and head of the observer, to keep the shadow on the same part of the retina, though even this would not insure its permanent invisibility. If this, therefore, were a phenomenon of indirect vision, the difficulty would consist in losing sight of the ramifications, whereas the difficulty really consists in seeing them; and this difficulty is so great with me, that I have never been able to see them again since I saw them at Cambridge.

of the candle. It cannot proceed from any imperfect transparency in any of the coats or humours of the eye, because it is seen in eyes that have the most pure and perfect vision. It must, therefore, be light produced by a physiological action, or light propagated from, or induced by the direct image of the candle upon the retina; and if this is the case, the explanation which I formerly gave of the phenomenon is likely to be the true one. The blood-vessels touching the retina will deaden, as it were, the part of the retina which they touch, or make it less sensible to the propagated light, and hence the blood-vessels will appear delineated in a fainter light than that which surrounds them. The distinctness with which the ramifications will thus be seen will vary with the intensity of the brown light, with the ever changing sensibility of the retina, and with the varying pressure of the blood-vessels themselves. If I could command the vision of these ramifications as Mr. Wheatstone can, it would not be difficult to institute experiments by which the preceding explanation could be cross-examined; and I therefore hope that he will resume the subject in reference to the facts and views which I have ventured to state.

Before concluding this notice I may mention, as connected with the subject, some curious phenomena which appear when we throw a condensed beam of light upon the retina so as to fill the whole eye. This may be done by holding near the eye a convex lens, about an inch in diameter, and an inch or so in focal length, so as to see its whole area filled with the light of a candle or lamp. If we move the lens backwards and forwards quickly, looking steadily at one point of the field, we shall see on each side of the axis of vision the ramifications described in my former paper and in the preceding pages. There are none of them visible within a certain space round the axis of vision; but in the axis of vision there is an irregularly illuminated or shaded circular spot, obviously corresponding to the *foramen centrale* of the retina; and in this spot, and for some distance round it, is seen a sort of net-work pattern, delineated in dark lines. This pattern* has sometimes all the regularity of one formed geometrically, with dark spots in the centre of each area, and the ground on which the pattern is seen is generally of a faint purple colour. But, what is more remarkable, the luminous field is crossed by exceedingly faint bands of red and green light perpendicular to the direction of the motion.

When the eye has not been fatigued by light, the luminous ground on which these phenomena are seen has a minutely granular appearance; and by the continued action of the light an apparent effervescence appears over the whole, as if each grain of light, or the minute spaces between the grains, were becoming more or less luminous in succession.—*Lond. and Ed. Philosoph. Mag. and Jour. of Science*, Feb. 1834.

* The very same phenomenon is seen, though less distinctly, when we look steadily at the moving or *flaring* summit of the flame of a candle.

PATHOLOGY AND THERAPEUTICS.

Encephalic Bruit de Soufflet in Compression, or Engorgement of the Organs contained within the Cranium.—No person as yet had the idea of applying the stethoscope to the head, and to study the sounds which are perceived by means of that instrument on the surface of the cranium. Dr. Fisher, of Boston, has just been engaged in researches of this kind, after having discovered what he calls an *encephalic bruit de soufflet*. He made known his discovery by rather a curious memoir which he read to the Society for the Advancement of Medicine at Boston.

As yet all he knows of the *encephalic bruit de soufflet* he has deduced from six facts, three of which recovered, and three terminated in death (*autopsie*). Five of the patients mentioned were attacked with chronic or acute hydrocephalus; one only had symptoms of inflammation of the brain, in consequence of a fall on the head, without any signs of dropsy of the brain. In a word, all these patients, who were infants, presented symptoms of compression or engorgement of the cerebral organs. In all, by applying the stethoscope or ear to any point of the surface of the cranium, according to Mr. Fisher, a well marked *bruit de soufflet* could be heard; the sound was more distinct under the anterior part of the sagittal suture than any other part.

Encouraged by the singularity of his discovery, Dr. Fisher stethoscoped the heads of several individuals of all ages, and the result of his researches is, that the *encephalic bruit de soufflet* does not exist in the normal state; that by auscultation of the head the sound of the air penetrating the nasal fossæ, the sound produced by deglutition and that of the voice, and also the sounds of the heart, may be easily perceived; that the *encephalic bruit de soufflet* exists in children when teething, but only in those who suffer much from dentition. What is worthy of remark is that that practitioner has observed the *encephalic bruit de soufflet* in two children attacked with hooping-cough, but only at the moment when the fit of coughing ceased, and whilst the accumulation of blood towards the head became evident from the redness of the face.

According to Dr. Fisher, that sound is produced in the trunks of the arteries of the cranium, while they are compressed by the brain, which takes place when that organ is pressed by fluid, or augmented in volume in consequence of inflammation. The calibre of these arteries is then diminished, the blood circulates with difficulty, and it is that impediment to the circulation, that friction of the blood against the parietes of the arteries, which produces the *encephalic bruit de soufflet*.

Dr. Fisher intends continuing his researches, and to use all his endeavours to appreciate the real value of that symptom in pathology, and what application may be made of it in the diagnosis of diseases of the brain.—*Archiv. Gen.* Jan. 1834.

Snow Blindness.—When the division Cordova marched from Cuzco to Puno, it halted at Santa Rosa. During the night there was a heavy fall of snow. They continued their march the next morning. The effects of the rays of the sun, reflected from the snow upon the eyes, produces a disease which the Peruvians call *surumpi*. It occasions blindness, accompanied by excruciating tortures. A pimple forms on the eye-ball, and causes an itching pricking pain, as though needles were continually piercing it. The temporary loss of sight is occasioned by the impossibility of opening the eye-lids for a single moment, the smallest ray of light being absolutely insupportable. The only relief is a poultice of snow, but as that melts away the intolerable tortures return. With the exception of twenty men and the guides, who knew how to guard against the calamity, the whole division were struck blind with the *surumpi*, three leagues distant from the nearest human habitation. The guides galloped on to a village in advance, and brought out a hundred Indians to assist in leading the men. Many of the sufferers, maddened by pain, had strayed away from the column, and perished before the return of the guides, who, together with the Indians, took charge of long files of the poor sightless soldiers, clinging to each other with agonized and desperate grasp. During their dreary march by a rugged mountain path, several fell down precipices, and were never heard of more. Miller himself suffered only fifteen hours from the *surumpi*, but the complaint usually continues two days. Out of three thousand men, Cordova lost above a hundred. The regiment most affected was the *voltigeros* (formerly Numancia), which had marched from Caracas, a distance of upwards of two thousand leagues.—*Memoirs of General Miller*.

Experiments on Cicuta and Henbane, by Professor Geiger of Heidelberg.—In the course of the chemical experiments recently made, the author has arrived at some very interesting results on the active principles of these two plants. The base of the poisonous principle of cicuta is an organic salt, which appears to commence a new series of these organic substances so interesting to know; in fact, this salt is volatile, and resembles an essential oil. The singular properties of this body, whether in its natural state or combined with acids, the rapid changes which it is capable of undergoing, and the brilliant colours which it presents when undergoing these changes, render it one of the most curious productions of organic chemistry. Its poisonous properties are most active, the least quantity taken internally produces paralysis; and one or two grains are sufficient to destroy the largest animal.

Another discovery of M. Geiger is, that of the active principle of hyoscyamus: this also is an organic salt, but fixed and capable of crystallizing. It forms crystallizable salts with acids, and has a disagreeable odour. Though not volatile, its poisonous properties are nearly as deleterious as that of the substance just now described, but

its effects are not similar, nor so rapid. Some animals to which the author administered this substance, even in a very minute dose, became languid, could not support themselves on their paws, were seized with convulsive movements, and died in the course of six hours. The property possessed by this substance of dilating the pupil is very remarkable; the smallest quantity applied to the eye of a cat produces a dilatation of the pupil for the twenty-four hours following, and the hundredth part of a grain prolongs this effect during seven or eight days.—*Rep. patent Invent.*, March, 1833, and *American Journal Med. Scien.*, May, 1832.

Treatment of Porrigo (Tinea), by M. Cazenave.—Monsieur Biett has sometimes employed with advantage, the following lotion, called Barlow's lotion :

R. Sulphate of Potash, two drachms (*gros.*)
 White Soap, two drachms and a half.
 Lime Water, seven ounces.
 Rectified Spirit, q. q. drachm. ℥.

The ointments with calomel, with oxide of manganese, in the proportion of one to two drachms per ounce of axunge, have succeeded well; the following also,

R. White Soap, two drachms.
 Sublimed Sulphur, two drachms.
 Axunge, one ounce. ℥.

Also that of Banger,

R. Litharge, two ounces.
 Calcined alum, one ounce and a half.
 Calomel, one ounce and a half.
 Axunge, two pounds.
 Venice Turpentine, a half pound. ℥.

None of all these agents is, however, so efficacious as the iodide of sulphur, introduced into therapeutics ten years ago, by M. Biett. The following is the formula which M. Biett used after a great number of attempts :

R. Iodide of Sulphur, twenty-four to thirty-six grains.
 Axunge, one ounce. ℥.

M. Dauvergne employs the following solutions:

Iodurated Solution.

R. Iodine, three drachms, (*gros.*)
 Iodide of Potassium, six drachms.
 Distilled Water, three ounces. ℥.

Sulphurous Solution.

R. Sulphuret of Potash, four ounces.
 Distilled Water, a half pound. ℥.

These solutions are to be mixed in the proportion of a drachm, a coffee spoonful of the iodurated solution, with half an ounce of the

sulphurous solution, that is nearly a table spoonful. The whole in a tea-cup of tepid or cold water, according to circumstances.—*Journal de Pharmacie et des Sciences Accessoires*, Jan. 1834.

Iodine Antidote for Strychnine, &c.—M. Donné states, that from closely observing a case wherein a woman's life was lately destroyed, experiments, the results of which are given by him in the October number of the *Annales d'Hygiene*, have led him to ascertain that tincture of iodine is the most effectual antidote for the vegetable alkalies, such as strychnine, brucine, &c. The iodine, bromine, and chlorine, form with these alkalies combinations, which in the dose of one or two grains exert no action whatever on the animal economy.—*Revue Med.*, Feb. 1834.

On the Poisonous Effects of the Cyanuret of Potassium, administered in the form of Enema.—Dr. Trouvé, Professor at the School of Medicine at Caen, communicated to me the following fact :

A patient affected with neuralgia of the trunk, was given three enemas, each composed of six ounces of water, and six grains of the cyanuret of potassium *moistened*, but still in mass. The first and third were administered cold : the second was heated with the sand bath, the bottle being kept corked. The three occasioned strong convulsions and violent spasms of the extremities ; the eyes were fixed, and the pupils dilated. After some time the patient recovered, and was able to sit up, which he had not done before for one year.

A fourth injection, composed like the preceding, only that the cyanuret of potassium was in a *pulp* (*en bouillie*), and so moist, that it adhered to the sides of the vessel, was administered to the same patient, who experienced no pain or any unpleasant sensation.

A fifth injection, given thirty-six hours after the fourth, was prepared with the same dose of the cyanuret *well dried*, taken from a vessel which had not been uncorked, and which scarcely exhaled any odour. The patient died, about one hour after, having had the following symptoms : general convulsions, palpitations of the heart, slow and difficult respiration, coldness of the extremities, dilatation of the pupils, and the eyes fixed.

This fact establishes a marked difference between the action of the cyanuret of potassium in a dry and humid state : the latter appears less active. Is it not probable, it may be on account of its decomposition by moisture ? The experiments of M. Pelouse militate in favour of that opinion. Did not that chemist prove, that by applying heat to a concentrated aqueous solution of the cyanuret of potassium, it was changed into ammonia, and formiate of potash, which is inert ? Would not similar compounds be formed by the prolonged action of moisture on the cyanuret of potassium ? However that may be, we must take it into account to explain the difference in the action of the quantity of water contained in the six grains of the cyanuret in pulp, (*en bouillie*), which necessarily diminished the pro-

portion of the cyanuret of potassium which was contained. — *Journal de Chem. Med.*, Jan. 1834.

Apparent Death which was continued for twenty Days, by Dr. Schmid.—A young man died in the hospital at Paderborn, who could not be buried until three weeks after he had breathed, at least to all appearance, his last breath. It was not till the twentieth day that the characteristic phenomena of death became manifest. The circumstances of the case were these. This young man had been a little time before cured of a tertian ague, when he re-entered the hospital, presenting some signs which caused an apprehension of phthisis, without however presenting any well-marked symptoms of this disease. In other respects, no disturbance in his health. On the day he died, his eyes were suddenly opened, and for some minutes we found an irregular beating at the pulse. Several small wounds resulting from cauterizations, to which we then had recourse to rouse him, suppurated the second, third, and fourth day. On the fifth, the hands of the body were turned back; from the fifth to the ninth day there exhaled from half the body an abundant sweat free from odour. Towards the end of the ninth day, there appeared over a considerable part of the dorsal region serous bullæ, similar to those of pemphigus. The limbs still preserved their natural suppleness, and on the eighteenth day, the lips still retained their red vermilion colour. For nine days the forehead continued furrowed with vertical wrinkles, and all this time the countenance preserved an expression never presented by the face of a dead body. The body was kept for nineteen days in a warm room; it exhaled not the least fetid odour, and there was observed on no part of its surface any cadaveric lividity. The emaciation was not very considerable, a circumstance, which if it had existed, might have served to explain these different phenomena.—*Hufeland, Journal der pract. Heilkunde*, Mai, 1833. *Archiv. Gen.*, Jan., 1834.

SURGERY.

Lacerated Perineum, Treatment of by Operation.—By far the most common cause of division of the perineum in females is, the injury sustained during a difficult labour; and this injury may proceed either from the excessive distention of the part, when the head of the child is making its escape, or from the mal-application of an obstetrical instrument, as of the forceps, lever, &c. Occasionally indeed this accident has arisen from an outward wound, or from a spontaneous and gangrenous ulceration; and in addition to these causes we may also mention, that a most complete destruction of the perineum has sometimes followed injudicious attempts to cure a fistula of the part. The extent of this injury may be very different; the perineum only may be lacerated; and this laceration may be either complete or par-

tial, the anus and its sphincter remaining entire; or a central perforation may have taken place in the perineum, and we know that in some rare cases, the child has actually been forced through this perforation. In another set of cases, we find that the perineum escapes, and the recto-vaginal septum is lacerated, or destroyed; and lastly, both parts may be injured together. It might very naturally be supposed that, when the anterior part only of the perineum, or as it is called, the fourchette, is divided more or less, the accident would be much more easily remediable, than when the sphincter ani is involved; but the very reverse is often found to be true; for indeed, the re-union effected by nature in the first case is always incomplete, and the female is constantly annoyed, more especially if she be young, with a state of parts, in which the vulva is considerably prolonged backwards, and has lost much of its contractility.

At present our attention will be limited to the most severe accident of all; that in which the whole extent of the perineum has been lacerated, either with or without an injury of the recto-vaginal septum.

This accident seems to be irremediable by unassisted nature; the edges indeed of the wound may cicatrize, but the healing is never accomplished throughout its whole depth. It forms a very frightful calamity; the vagina and rectum are laid into one, and the discharges of the latter are often voided by the former passage. It is however by no means unfrequent, that the female becomes again pregnant, and her accouchement may be the more easy and rapid.

M. Roux knows an English lady who suffered a complete laceration of the perineum in her first labour, and afterwards gave birth to twelve children successively, the accident remaining unrelieved all the time. He thinks it very probable that a great many females may be the subjects of this disgusting calamity, who are ashamed to avow its existence.

In the worst cases the state of the patient is truly miserable; the power of retaining the *fæces*, &c. may be utterly lost, and she is sometimes constantly harassed with the desire of evacuation, and before she has time to prepare for it, the vagina and adjacent parts may be in a moment deluged with it. She is thus forced to seclude herself from all society, and it cannot be surprising that her general health soon languishes and decays.

How gratifying must it be to a feeling surgeon to be able to rescue a fellow being from such distress! The earliest case on record, where an attempt was made by the surgeon to repair the loss of the perineum, is one which occurred to Guillemeau, the disciple of Ambrose Paré; the interrupted suture was employed, and the operation was quite successful. Subsequently to his time, the operation, although spoken of and recommended by some writers, was seldom or never attempted, until about the close of last century; when two French surgeons, MM. Noel and Saucerotte, performed it with complete success, by means of the twisted suture. Since that period it has been put in practice about half a dozen times in France; but in

mos of the cases, with little or no benefit to the patients. The English surgeons seem to have altogether neglected making any attempt in this field of surgery; and the Germans, though of late they have been ample in their descriptions of the best method of operating, have not contributed any essential improvements. M. Dieffenbach, of Berlin, has been most zealous in the cause; but with some of his opinions we cannot agree; he tells us that there is no chance of our being able to effect a complete union of a divided perineum, unless we previously make two parallel incisions along the sides of the vulva and perineum, in order that the parts may yield, and thus allow themselves to be kept in easy and natural contact. We shall see hereafter that this preliminary step is quite unnecessary, and ought, therefore, to be altogether abandoned.

It was the following very interesting case which suggested to M. Roux, that improvement in the operation, from which he anticipates the most agreeable results in future.

CASE I. A young lady, twenty-two years of age, came from Normandy to Paris in December, 1831, for the purpose of having M. Roux's advice respecting a division of the perineum.

She had been married to a medical man, when she was only nineteen years old; and very soon after marriage had become pregnant, so that her accouchement came on just as she reached her twentieth year. The labour was a painful and protracted one, and required the use of the forceps for its completion; unfortunately the perineum throughout its whole extent was lacerated, and the recto-vaginal septum, for about half an inch, was also torn. This distressing accident was now of two years' standing; and nature had done nothing to repair the injury. M. Roux, on examining the parts, found that the division was exactly in the median line of the perineum; its edges or lips were quite smooth, soft, and free from any callosities; so much so indeed, that a person might have supposed at first sight that it was a congenital defect. The anus and vulva formed but one common outlet; and hence the condition of this interesting patient was most loathsome and afflicting. In order that the frequent desire of voiding the intestinal discharges might be lessened, she had long accustomed herself to take different preparations of opium; and the effect of these had been, at least in one respect, most soothing; for by regulating the doses she could retain the bowels in a constipated state, for almost any length of time; but notwithstanding this relief, the patient was so afraid of the desire ever coming on unexpectedly, that she quite secluded herself from all society, and her life was spent in wasting melancholy. Fortunately her constitution was decidedly good, and the circumstance of her having acquired the power of confining the bowels, for almost any period which might be desired, was favourable to the success of any operation. The operation was performed in January, 1832, and as M. Roux had at this time no experience in such cases, he followed the practice which he knew had been recommended by most surgeons. The suture which he employed was the twisted one. After paring

very carefully the edges of the cicatrized lips of the fissure, he transfixed them with four long needles, introducing these at least one inch from the edges of the wound, so as to prevent the risk of their being loosened by ulceration. No lateral incisions were made, because the part did not appear to be much stretched.

There was not an unfavourable symptom after the operation; the urine was drawn off by the catheter—the most strict regimen was enforced, and the bowels did not act. On the seventh day, M. Roux determined to remove the needles, as the appearance of the wound indicated a re-union throughout its whole extent; but, most unfortunately, this appearance was fallacious, and the adhesion was nothing but a simple agglutination. Two days after the removal of the needles, the wound was quite disunited, and the part, in the course of a short time, was in the same condition as it had been before the operation.

A second attempt was resolved upon, and the patient, although naturally enough disheartened by the failure of the first, was too anxious to submit to any rational experiment, which promised a chance of relieving her from her miserable state.

M. Roux, from reflecting upon the first operation, was inclined to attribute its failure to the employment of the twisted suture, which, acting almost solely on the outer edges of the wound, did not keep up an accurate contact of the parts more deeply seated. That a union did not take place, therefore, no one need be surprised, especially when it is considered that the parts were continually kept moistened with the vaginal discharges.

The preliminary steps of the second operation were quite the same as those in the first. Four strong double ligatures were passed through the lips of the wound by means of curved needles, introduced on the one side from without inwards, and on the other from within outwards; and two pieces of bougie were then laid along the two edges, and accurately retained in their position, the one being received into the loops of the ligatures, while their loose ends were tied firmly over the other. Those who have employed the quilled suture, know that a wound always gapes somewhat after its application, and for a very obvious reason—because the pressure is exerted chiefly on the deeper part, and very partially on the outer edges. M. Roux, having calculated on this, took the precaution of inserting a single fine silk ligature along with each double one; and, when he had adjusted the quilled suture, he then tied these single ligatures as after ordinary operations. On the seventh day, the pieces of bougie, and, at the same time, all the ligatures were withdrawn, and the agreeable discovery was made, that a firm and solid union had taken place. Every day successively, for ten or twelve days, the consolidation of the part became stronger and more secure, and the bowels, most fortunately, were not once disturbed until the twenty-second day after the operation; and, although the evacuation then was copious, and of a hard consistence, and accompanied with so much pain and forcing down, that it was necessary to assist its expulsion by pressure of

the finger within the vagina, the re-union of the parts had by this time become so complete, that no injury whatever was sustained.

[It would certainly have been prudent to have obviated such a state of things by emollient enemata.—REV.]

At the period when this patient left Paris, there was still a small aperture of communication between the rectum and vagina, immediately above the sphincter ani; the fæces did not, however, pass through it, and M. Roux was informed afterwards that it quite healed up.

The result of the preceding case has been most gratifying; for it appears that the patient was speedily restored to the enjoyment of connubial intercourse, and within five months after the operation became pregnant, and was, in due time, delivered safely of a full grown child, the perineum escaping entire and uninjured.

CASE II.—A girl, twenty-one years of age, was admitted into the Hospital de la Charité in March, 1833. She had become a mother nearly two years before, and so severe had been the delivery, that the perineum had been torn completely through.

Before undertaking any operation, M. Roux subjected her to a very spare diet for several days, in order that there might be little occasion for relief of the bowels. The steps of the operation were the same as we have described in the former case, and a similar after-treatment was rigidly followed. But, as it could scarcely be expected that the bowels would be quite so accommodating, in the present instance, as not to act until two or three weeks elapsed, and until the union might, therefore, be solidified, M. Roux, on the evening of the sixth day, ordered an emollient enema to empty the gut. On the following morning, the ligatures and bougies were removed, and it was found that a very satisfactory union had taken place. By the end of the third week, the adhesion was perfect throughout, except at the deepest part of the recto-vaginal septum, where a small fistula remained, and gave exit occasionally to intestinal gas; but this also gradually contracted, and had become quite minute, when the girl left the hospital.

The third case occurred in a woman, twenty-nine years of age, mother of five children; her last accouchement had been lingering and severe, and the application of the forceps had induced a complete laceration of the perineum. M. Jacobson, of Copenhagen, and many of the most eminent surgeons in Paris, were present at the operation performed by M. Roux on this woman, and they had afterwards an opportunity of ascertaining the admirable cure that was effected.

The last successful operation was performed on a lady of rank, mother of three children, who had suffered from this distressing calamity for upwards of two years, but who had never mentioned its occurrence to any one, except her accoucheur, until she consulted M. Roux. The steps of the operation were quite the same as he had followed in the other three cases, and a most complete success rewarded his attempt.

The only instance in which he has failed occurred very recently;

the patient died on the tenth day. The particulars of the case are worth recording.

The woman was forty years of age, and, in her, the destruction of the perineum had been the result, not of a difficult labour, but of an attempt which had been made by a surgeon to cure a fistula ani, communicating with the vagina. In consequence of this, the parts must necessarily have been more or less diseased, and, moreover, the patient laboured at the time under a complete prolapsus of the rectum: whenever she stood erect, or coughed, sneezed, or in any way exerted herself, the inverted gut was forced through the large gap in the perineum, forming a tumour as big as a man's fist; it might, indeed, be returned, but no means that had been used could keep it up permanently. When this patient entered the Hôpital de la Charité, she was suffering from continued fever, accompanied with diarrhoea, distress of the abdomen, and other symptoms, which indicated some inflammatory disorder of the mucous surface of the bowels. During a period of four weeks, an appropriate treatment quite recovered her; and then, at her own earnest request, M. Roux proceeded to the operation. Unfortunately, on the third day afterwards, fever again set in, the abdomen became very tender, and the diarrhoea returned. The wound did not exhibit any appearances of the adhesive process, and the ligatures had caused ulceration. On the seventh day, the bougies and threads were removed, and, on the ninth, the disunion was complete; on the following day she died. It is quite reasonable to suppose, that the irritation of the ligatures reproduced those symptoms of intestinal disturbance which ultimately proved fatal; and M. Roux is candid enough to avow, that perhaps he did not delay the operation for a sufficient length of time after the first illness.

In conclusion, M. R. offers some remarks as to the proper period after the occurrence of the accident for the performance of the operation. When it has taken place during parturition, (and this is by far the most frequent cause,) it would not be judicious to attempt by art the union of the laceration for at least two or three months; the highly nervous and impressionable state of constitution in a parturient woman, the recent extreme distention of the parts, the copious flow of lochia, &c. are potent reasons against an early operation. Let the wound, therefore, be left to Nature's efforts, until these objections no longer exist, and let the medical man be satisfied with the gentlest treatment.

It will be found that, in most cases in which the operation is performed, a very considerable degree of dysuria takes place for some days; the catheter ought, therefore, to be carefully introduced twice, or oftener, a day. In every one of M. Roux's successful examples, the lips of the wound close to the anus, or recto-vaginal septum, were found to be disunited, although the rest had healed at the time when the ligatures and bougies were removed. There always remained for a week or more, at this part, a fissure, not unlike that which we make in operating for fistula ani; this fissure gradually, however,

contracted, and the anus, into which a small oiled tent should be introduced, quickly recovered its healthy condition. The part of the wound most slow in healing is the recto-vaginal septum, and the progress, in some cases, will be found very tedious, in consequence of the extreme difficulty in preventing the passage of the fæces, or of the intestinal gases, from the gut into the vagina. But even this, in course of time, and with the assistance of the judicious surgeon, will contract more and more until it finally closes entirely.

The success which M. Roux has obtained, in a set of cases which are by no means unfrequent, and which, hitherto, have too often baffled surgical relief, encourages him to hope, that the operation which he has recommended will become as common, and as fortunate, as that of staphyloraphy, which he was the first to perform in 1819, although, since that time, no fewer than sixty-five cases have presented themselves to his notice.—*Med.-Chir. Review, and Jour. of Prac. Med.*; edited by Dr. Johnson, April, 1834.

Hearing, through the Apertures made by the Trephine.—While watching the effects of the operation of trephining, in several patients at the Hôtel des Invalides, M. Perier, an assistant surgeon, has discovered, or at least has imagined that he discovered, that they were all conscious of a sensation of a very unusual and constant noise in the part. We have seen the following experiments made at the clinique of the Baron Larrey, and in the presence of the philosopher, M. Savart. The ears of a patient, on whom the operation had been performed, having been well stopped, while the rest of the head was left unincumbered and free, it was found that the sense of hearing was not at all affected, but that he could still perceive every sound quite distinctly, and the more so, when the sounds were directed perpendicularly downwards on the surface of the cicatrix. Even at a considerable distance sounds could be satisfactorily enough distinguished to enable the person to hold conversation with another. The beats of a watch held at a short distance from the cicatrix, were also made out.

Now if, while performing any of these experiments, the artificial aperture in the skull was well covered and compressed with the hand, while the ears remained plugged, the perception of sounds was immediately obstructed.—*Ibid.*

Purification of Theatres of Dissection, &c.—A special commission was lately appointed for the purpose of ascertaining the best method of disinfecting anatomical theatres of their stench and unwholesome effluvia. They tried a multitude of expedients, but found that the use of simple charcoal powder was much the most efficacious. Some of this powder was blended with and sprinkled over the putrid contents of the bowels one day, and on the next, it was always found that their offensiveness was in a great measure removed; and if the students rubbed their hands well with the charcoal before they washed them, all unpleasant smell was most cer

tainly got rid of. This practice has been tried extensively at the dissecting amphitheatre of the La Pitié Hospital, and from its simplicity and efficacy is now constantly adopted there.

One great advantage of the charcoal is that it is a harmless substance, and that it does not even cause the steel instruments to rust, which unfortunately is apt to be the case, if the preparations of chlorine are used as a disinfecting agent.—*Ibid.*

Traumatic Tetanus.—A midshipman, on board of an Indiaman, got wounded in the left foot by treading on a rusty nail, 14th August. He kept his watch during a cold, rainy night, and, by eight o'clock next morning, presented symptoms of trismus. An opiate with camphor was exhibited, but the tetanus increased, and the limb was cold, and as he described it, dead and powerless. The pulse was 120, and his situation altogether alarming. It was proposed to the surgeon of the ship, by Dr. Murray, the narrator, to tie the posterior tibial nerve, and thus cut off communication with the wound. This was done, and although almost incapable of articulating a word the moment before, he immediately opened his mouth with an exclamation. His countenance presented a remarkable change for the better, and he said he felt life returning to his leg. The original wound was dilated, and a poultice applied. His bowels were now copiously opened, after which he fell into a sound sleep, which continued four hours. The tetanic symptoms, however, though mitigated, did not disappear, and it was deemed advisable to bleed the patient to syncope, two days afterwards. By the 18th of August, the tetanic symptoms had nearly disappeared, and he convalesced rapidly from this time. This case is related by Dr. Murray in the sixth volume of the Calcutta Medical and Physical Transactions.—*Ibid.*

Neuralgic Affections of Stumps.—The following case occurred in St. Bartholomew's Hospital, and is published in the Medical Gazette, No. 325, by Mr. Crookes, the house-surgeon.

“ Sarah Slyfield, about eighteen years of age, was admitted into St. Bartholomew's in July last. She had undergone amputation of the right thigh in the country about nine months before her admission, for disease in the knee-joint, the effect of an injury received several years previously. When received into the hospital, she stated that the pain in the stump had come on within a few days of the removal of the limb; and that, although the part had been carefully examined, and means adopted for her relief, it had continued to increase, almost without intermission, up to the present time. Shortly after the wound had healed, six months after the operation, a small lump formed in the extremity of the stump, which was opened, and several pieces of dead bone extracted, but without affording any relief. Menstruation had commenced about the age of sixteen, and continued regularly up to the period of the operation, but has since only occurred once. Her person was stout, with a hale and somewhat bloated countenance, and she complained of nothing but severe and

incessant pain in the stump. Examination of the part much increased her distress, as the slightest touch in any point of its integument gave her pain. The limb was naturally large from fat, but without any swelling or redness; and the extremity of the bone seemed well covered by soft parts. She was bled to sixteen ounces, and ordered purgative and saline diaphoretic medicines, and to apply linseed meal poultices.

“ This painful affection, which was so severe as almost entirely to prevent the patient getting rest, continued gradually to increase for about four months, in spite of the adoption of the most active remedies. Local and general bleeding, counter-irritation by moxa, narcotics, and the application of sedative plasters to the stump, with tonics and antispasmodics, were all tried in vain; and the patient began to entreat to have the stump removed, to procure her, if possible, some alleviation of her sufferings. During this time she also experienced repeated attacks of hysteria; and was once seized with pain and acute sensibility of the integuments of the abdomen, which at first excited a suspicion of peritonitis.

“ As it was suggested by some that there might be suppuration in the extremity of the stump, a puncture was made into it, and the knife carried to a considerable depth, but without evacuating any matter, or producing any beneficial effect: it was therefore resolved to perform a second amputation of the limb.

“ *Operation.*—On account of the shortness of the stump it was impracticable to employ the tourniquet, and compression was made on the artery, as it passes beneath Poupert's ligament, by an instrument ordinarily used for that purpose. The muscles on the outer part of the thigh were first divided, and those on the inner, including the artery, last, in order that the vessel might be secured as soon as cut through. The bone being sawn through, and several small vessels tied, the ischiatic nerve was drawn out, and about an inch of it removed; some of the branches of the crural nerve were also cut shorter. Fifty minims of laudanum were given after she was taken to bed, as she was suffering great pain.

“ *Examination of the Stump.*—The extremity of the ischiatic nerve was bulbous, and of an almost cartilaginous hardness, and from this bulb there was continued in various directions a layer of dense cellular tissue, connecting it with the bone, the muscles, and the cicatrix. One of the filaments of the anterior crural nerve, perhaps the nervus saphenus, had also a bulbous termination, comparatively much larger than the ischiatic, and was similarly connected with the surrounding parts. The extremity of the bone appeared free from disease, nor was there any suppuration in its neighbourhood.

“ From the time of the operation up to the period of her discharge from the hospital, with the exception of a slight recurrence of the painful sensibility of the integuments of the abdomen, she continued to go on well, never having experienced any pain in the stump, or any unfavourable symptom. The remaining portion of the limb was very short, but sufficient to permit of the adaptation of the

ordinary wooden leg, with the additional security of a strap to pass over the shoulder."—*Ibid.*

On the Branchial or Gill-like Openings in the Neck of the Human Fœtus, as a Cause of certain Malformations.—M. Ascherson, the author of this memoir, lately published in Latin at Berlin, has called the attention of his medical brethren to a curious congenital anomaly; or "*vitium formationis*," which he has observed in several persons. The number of cases altogether amounts to eleven; and most of them occurred in female children, of a scrofulous, or at least of a lymphatic constitution. The authenticity of most is guaranteed by the testimony of that able physiologist Rudolphi. The following may be given as a brief description of the disease:—On the anterior and lateral part of the neck there is observed a fistulous opening, which is situated generally in that triangular hollow between the clavicle and the two points of insertion of the sternomastoideus; but sometimes it is at the inner edge of this muscle. It is much more frequently found on the right than on the left side; and if there should happen to be one on either side, that on the right is always larger and placed somewhat higher up than the other one. The aperture is invariably very narrow; occasionally scarcely visible, but at other times it is surrounded with a red circle, or it may project like a papilla. It generally follows the movements of the pharynx in deglutition, and when this is the case, we observe a transverse furrow, at the bottom of which is situated the fistulous opening. If a probe be introduced, it may perhaps be pushed forwards a little way, but in most of the cases it is stopped very soon, in consequence of the sinuosity of the canal. In one case, fluid, injected at the outer opening, passed into the pharynx, and the patient was sensible of its taste; and in another, the attempts made to cure the fistula in this way were followed by disagreeable consequences, such as swelling of the neck, smarting pain, and the sensation as if a foreign body was sticking in the pharynx. On no occasion was any air ever observed to escape from the opening, even when the effort of expiration was strong, while the mouth and nostrils were kept closed. The discharge from the fistula was sometimes viscid and clear, and at other times, more of a purulent appearance; and it was remarked that in the latter case the quantity of the discharge was always more profuse. Although this disease be congenital, it may increase after birth beyond its original extent. Eight of the cases seen by Dr. Ascherson, occurred in females, and three in males. These fistulæ now described have some analogy with the tracheal fistulæ recently discovered and explained by M. Dzondi; but the origin and the anatomical characters of the two are very different. In order that we may compare them, we have extracted the following remarks from Dzondi's narrative.

"At the anterior part of the neck, about the middle of the concave edge of the thyroid cartilage, there is found a small round opening, of about a line in diameter; its edges are neither red, tumefied, nor surrounded with any fleshy rim. It is not painful on being

touched; and when firmly compressed, several drops of a puriform fluid may be made to flow out. A probe cannot be pushed very deep, in consequence of the winding track of the fistula, and on no occasion can it be introduced into the trachea, although a few bubbles of air almost always escape upon any forcible expiration."

These tracheal fistulæ may be associated, or occur in connexion with other congenital anomalies or irregularities of formation; especially with those which are denominated "monstrosities from asymphysis," that is, from an incomplete junction of the two lateral halves of the body. We cannot, however, take the same view of those described by M. Ascherson, because they are not situated in the median line of the body; and as they have no communication with the air-passages, we must necessarily infer that their origin is not similar. He is of opinion that they should be regarded as proceeding from some anomaly or aberration of the *nisus formativus*, congeneral with those which cause an arrest of the development of the foetus, during one of those phases which it successively passes through, before it reaches its perfect state. To one of these transition forms belong the branchial fistulæ discovered by Rathke, first in the young of the pig, horse, hen, water-snake, (*coluber natrix*), and lizard, and afterwards in a human embryo, about seven or eight weeks old. These fistulæ or tubes consist in from six to eight apertures, arranged symmetrically on either side of the neck, opening into the pharynx, covered externally with a sort of operculum, and exhibiting on their inner surface several arched lamellæ. Rathke compares these apertures with the branchial apertures of the shark; and a beautiful confirmation of this opinion may be derived from the identity which exists in the vascular arrangements of fishes and of the early chick, as clearly made out and described by M. Huschke of Dresden. This anatomist publicly demonstrated that the aorta of the young chick gives off six branches, which pass on to the inner surface of the branchial arches, (or those lamellæ which are considered as rudimentary branchiæ,) and which afterwards communicate with, and terminate in, the descending aorta. Bæer has verified the existence of these branchial apertures in the foetal dog and rabbit; and his observations have been confirmed by Burdach, Muller, Allan Thompson, and Becker. M. Rudolphi mentions having seen at Stralsund, an infant, in whom the closing up of a fistula of this sort, brought on aphonia, epileptic convulsions, and other alarming symptoms, which gave way only when the ulcer was re-established, and the discharge permitted to flow. In one of the cases related by Dzondi, the healing of the fistulous opening was followed by a train of evils which finally proved fatal.—*Ibid.*

Section of the Tendo Achillis, as a Means of Cure for Club-foot (pied bot), by Dr. L. Stromeyer, Professor of Surgery, &c. Translated from the German, by Dr. G. Richelet.—The section of tendons for the cure of certain deformities, had already been proposed; but to Delpech belongs the honour of having raised it from

oblivion, and submitted it to rules. According to him the tendon to be divided should not be exposed; the section ought not to be made by an incision parallel to that in the skin, for it might cause the sloughing of the tendon. Immediately after the division of the tendon, the two ends ought to be brought into contact, and maintained by a convenient apparatus, until they become consolidated; that reunion taking place by the interposition of a fibrous substance of new formation, you must, before that substance has obtained all its solidity, elongate it by means of extension gradually augmented; lastly, when it is sufficiently elongated, the part must be maintained in its situation, until the intermediate substance has acquired all its solidity.

Such are the principles on which the operator ought to act. However, the surgeon of Montpellier does not seem to have completely fulfilled the indications laid down by himself. In the case described in his work, he divided the tendo achillis, in a young man, aged nine years, in the following manner: the patient being laid on his face, a bistoury was passed behind the tendo achillis, so that on each side of the tendon a wound of about an inch in length was left. The instrument was immediately withdrawn, and a convex knife was introduced, with the cutting edge towards the tendon, by which means it was divided transversely from behind, without dividing the integuments situated immediately above. But by that operation the exfoliation of the tendon was not prevented. The sides of the tendon became adherent to the bottom of the cicatrices, which had formed slowly in consequence of the suppuration, the movements and flexion of the foot gave rise to a disturbance of the skin: besides which it gave great pain.

Dr. Stromeyer has modified in the most ingenious manner the operation of Delpech, and has experienced remarkable success, which may be seen in the two following cases:

CASE I.—Georges Ehlers, aged nineteen, was attacked in his fourth year, without any known cause with a deformity of the left foot, which had progressively augmented. The therapeutic means, and the apparatuses at first employed produced a little amelioration, but afterwards the disease made such progress, that walking became quite impossible. The excoriations, and deep suppuration which followed these attempts at reduction, brought on caries of the fifth metatarsal bone, the cure of which was delayed for several years. The parents then renounced all treatment, and had made for their son, who was at this time fourteen years of age, a wooden leg which had its resting point on the sciatic tuberosity, and which was fixed to the leg by straps and buckles. With this apparatus he could walk tolerably, although slowly, leaning on a stick in dry, and on a crutch in wet weather.

When he consulted Dr. Stromeyer, in October, 1830, he presented on the left side a club-foot, (*pied bot*). The nails were strongly turned inwards and downwards; the external side of the foot was entirely under the axis of the leg, and all the foot was in

such a state of extension, in consequence of the contraction of the gastrocnemii muscles, that the sides of the foot formed a straight line with the anterior surface of the leg. Above the external border of the foot, the traces of two callosities existed, which were formed during the time the patient rested on that part while walking at the commencement of the disease. On the fifth metatarsal bone was a cicatrix of about an inch long, adhering to the bone. The mobility of the foot was very limited: the leg much emaciated. The calf, which was drawn up into the bend of the leg, was scarcely perceptible, unless the muscles which constituted it were put into action by the movements of the leg. The continued use, during five years, of the wooden leg had determined, in the articulation of the knee, a considerable deviation outwards. The right leg was very muscular; but the right foot presented a considerable dorsal convexity, which made it appear shortened, and resembling a horse foot, (*pied équin*); that form had no unpleasant influence on its function. The author cites that last phenomenon, because it proves, according to him, that the cause of the deformed foot is situated in the spinal marrow.

The mobility which still existed in the articulation of the foot, and the firm resolution of the patient, determined Dr. Stromeyer to make some new attempts. The use of the extending apparatus effected, at the end of three months, only the development of the sole of the foot, and an amelioration in the position of its sides. When a greater force was used, there resulted excoriations in the sole of the foot, which were difficult to heal. It was then they determined on the division of the tendo achillis, as a last resource.

The operation was performed on the 28th February, 1831; the patient being placed on a table, before the operator, with his left side towards him, an assistant fixed the knee, another held the foot, and flexed it so, that the tendon was strongly stretched. A sharp pointed bistoury, very narrow, and bent so that the cutting edge was convex, was passed in two inches above the insertion of the tendon, between it and the tibia; the back of the bistoury being turned towards the bone, and the cutting side towards the tendon; which was divided by the introduction of the instrument; the division was made with a perceptible noise. The indication of making the external wounds as small as possible, so as to prevent the entrance of the air, exfoliation of the tendon and suppuration, was perfectly fulfilled, for only the point of the instrument traversed the opposite side, without making a bleeding wound, and the wound where it entered was only the size of the blade. The division of the tendon was favoured by the extension which an assistant made by means of strong flexion of the foot: the loss of blood was very insignificant. The retraction of the tendon was very inconsiderable; however, by flexion of the foot, there was a separation of three quarters of an inch between the two ends, without the form of the foot being much ameliorated. In the extension of the foot, that interval disappeared, and the ends of the tendon were in contact. The two small wounds were covered with two pieces of adhesive plaster. On each side of the tendon was

placed a long narrow compress, maintained by the first turns of a bandage which encircled the foot in the figure of 8, and was destined to keep it in the extended position. The employment of a splint was not considered necessary, for the atrophy of the muscles did not produce any fear of the displacement of the ends of the tendon. The limb was rested on a cushion.

The third day, the wounds were healed; the ends of the tendon appeared a little swollen and sensible to the touch: on the internal malleolus, there was a little ecchymosis. The movements of the foot produced no influence on the superior portion.

The sixth day, the ecchymosis was dissipated. The ends of the tendon were re-united, so that the moving of the foot with precaution, put in motion the upper portion of the tendon. The swelling corresponding to the re-union of the two ends was scarcely perceptible; still that place was the seat of great sensibility.

The tenth day after the operation, that sensibility was in a great measure dissipated; the two ends were already so solid, that the patient could, by the voluntary movements of the foot, put in motion the gastrocnemii, and stretch the tendon achillis. The touch gave but an imperfect idea of the distance that existed between the two ends.

Dr. Stromeyer thought the time to use extension was come, and he applied his apparatus, the description of which is given lower down. At this time the foot formed with the leg a very obtuse angle, and the machine for extending was put in motion, but with the greatest precaution, for fear of tearing the new adhesions, and of causing pain in the seat of the cicatrix. At the end of a week the screw could be turned more frequently, without causing pain. Eight weeks after the operation, the foot was at right angles with the leg. At this time the patient wore a buskin, furnished on the external side with a plate of iron, which had a joint opposite the articulation of the foot. At this joint was a screw, the revolution of which could diminish the angle which the foot made with the leg, without diminishing the mobility of the joint. With this buskin the patient was soon able to walk in his chamber, with the aid of a stick; however, as the leg was weakened by inactivity for several years, it soon became fatigued and cedematous. From that moment he took exercise daily, first in his chamber, then in the garden, and acquired so much strength and assurance, that, fourteen days after the first attempts, he was enabled to walk a quarter of a league and return. At first he wore the extending apparatus every night, and the buskin during the day; but as the application of that machine was painful, in consequence of the cedematous state of the limb produced by walking, he wore the buskin night and day, removing it twice a day, for the purpose of rubbing the foot with camphorated spirits. During the night, by means of the screw, the angle which the foot formed with the leg was diminished. You could not by the touch accurately distinguish the length of the intermediate substance newly formed, for the points where it commenced could not be ascertained, and the

tendon was felt to be narrower only opposite the section. The foot of itself could be placed at right angles with the leg. The oedematous swelling did not yield for two months to the use of lotions and spirituous baths. The gastrocnemii muscles increased during that period, but they were situated much higher up than in the right leg, which proves that the straightening of the foot was not due to the elongation of the muscles, but to the intermediate tendinous substance. The deviation of the knee disappeared with surprising rapidity; there was no trace of it six months after the operation.

When Dr. Stromeyer drew up this case a year and a half had elapsed since the operation; the form of the foot had not ceased to improve; it had attained its natural situation, without any inclination of the point inwards; the size of the limb, although less than the opposite one, had much increased. The tendency to oedema had entirely ceased. At the end of six months the patient did not require the use of the buskin, furnished with a plate of iron. He walked with a common boot without a stick, and no person could by his walk have any suspicion of the former state of his foot, for he could put it on the ground without any effort, with the point directed forwards, even while walking rapidly.

A severe contusion, during the course of the preceding summer, by the pressure of a narrow new boot, forced the patient to keep quiet for some weeks. However, the position of the foot was in no way altered, which proves that the intermediate substance had no tendency to retract, and that the cure could be considered as permanent.

CASE II.—M. B. Blumenthal, aged thirty-two, having heard of the favorable result of the former case, came to consult Dr. Stromeyer in May, 1832, for a club-foot of the left side. This disease had its origin in convulsions, which the patient had at the age of eighteen months; it had gradually increased, without, however, at first preventing his walking, as he used to follow his occupation as cattle dealer: but, after some time, walking became difficult, and so painful that he was obliged to keep his bed from time to time. A hernia, on the right side, succeeded in 1831, which forced the patient to renounce all fatigue.

When he consulted Dr. Stromeyer, the distorted foot was nearly in the worst state. The exterior side of the foot was so turned downwards and inwards, and also the nails, that in walking he rested on the front of the foot, when there was formed a thick horny callosity. The point of the foot, which was completely turned inwards, in walking, irritated the skin which covered the internal malleolus of the opposite side. The heel, which was drawn up, did not touch the ground; the tendo achillis was narrower than that of the sound limb; the muscles had not lost much of their size, but they were drawn up higher in the bend of the leg; the articulations were still moveable, both by the will, and by a foreign impression; however, the movements of the foot were very limited. That mobility, the slight atrophy of the leg, the determination of the patient, &c., de-

cided Dr. Stromeyer to attempt the operation of dividing the tendo achillis.

Before commencing the operation, Dr. Stromeyer took off the hardened cuticle, which covered the back and side of the foot, and which, in some places, was a quarter of an inch thick, then, having enveloped the foot in a woollen stocking, he placed it in an extending apparatus. He had only in view the replacing the foot in the axis of the leg. It was only after having arrived at that, the 12th June, 1832, that he divided the tendon.

The operation was performed as in the preceding case, only that the bistoury was introduced three inches above the insertion of the tendon, so as to remove, as much as possible, the compression of the apparatus from the wounds. The tendon being thicker than in the first patient, the simple introduction of the bistoury was not sufficient for its division, it was obliged to be passed about half an inch without the opposite side; the division of the tendon was made with an audible crack. After its division, when an attempt was made to flex the foot, the form of which was in no way modified, the two ends of the tendon were separated from one another nearly half an inch; by the extension of the foot they were brought into immediate contact. The two external wounds which were not larger than the bistoury, and from which very little hemorrhage succeeded, were united by means of adhesive plaster. In this patient, however, in consequence of the development of the muscles of the leg, a strong restraining power was placed on the anterior aspect of the foot, to prevent all voluntary movement. A short time after the operation, and principally the following night, the patient suffered from severe cramps in the calf of the leg, which came on when he slept, and which forced him to remain awake all night.

The second day, the ends of the tendon were not in contact; to prevent any muscular contraction, a bandage was applied over the whole leg, but this means completely failed, and this night was passed as the preceding. This unsuccessful attempt determined Dr. Stromeyer to abandon the bandage, and the cramps were not renewed.

The fifth day, the wounds were cicatrized, and the two ends of the tendon united, of which they were assured by moving the foot. But, to allow time for the reunion to become more solid, the extending apparatus was not used until the tenth day. Under the influence of this apparatus, the position of the foot improved so rapidly, that at the end of five weeks the foot formed a right angle with the leg. The patient was then made to use the same buskin as in the former case: but its application was not rendered painful by any oedema. In the commencement exercise caused pain in the seat of the cicatrix, but it soon disappeared.

About ten weeks after the operation, at which time Dr. Stromeyer lost sight of the patient, the foot formed a right angle with the leg; the sole of the foot rested horizontally on the ground in walking; the back of the foot was still very convex, which was manifestly owing to a curvature of the metatarsus; when he walked with care the

foot was carried outwards; when he walked quickly, the point of the foot deviated a little inwards, but not so far as to injure the opposite side. The muscles of the calf were increased in a remarkable manner, but they were still situated higher up than in the right leg. As in the former case it was impossible to appreciate the length of the intermediate substance, which, however, appeared weaker than in the former case in the situation of the section.

In these two cases, the second of which has been abridged in consequence of its great similarity in many respects to the former, talent and perseverance succeeded, in a very short interval of time, in overcoming a deformity which might have been reasonably regarded as incurable, as much on account of its long standing, as on many other unfavorable circumstances. The extending apparatus, invented by Dr. Stromeyer, is composed of a splint a little longer than the leg, and terminated below by a rounded hollow, destined for that part of the leg which corresponds to the external wounds and that of the tendon, as well as the projection of the heel. Between the two sides which bound the hollowed part, a moveable piece of wood, in the shape of a sole, is situated, and which consequently could form greater or less acute angles with the splint. At the lower end, the two sides are united by a kind of roller or moveable cylinder, furnished at one extremity with a cog wheel (*roue dentée*,) which is turned by means of a small lever. A cord which passes underneath the foot-board, near its anterior third, passes up on each side, passes over two pulleys fixed laterally near the middle of the splint, reascends, and passes round the moveable cylinder. After having enveloped the leg in a convenient manner, and having put on a woollen stocking, it is adapted to the splint, which corresponds to the posterior surface of the leg, by leathern straps; the sole of the foot rests on the moveable foot-board, which at first forms with the splint (in the case of a club-foot) an obtuse angle above, and an acute angle below; the foot is firmly fixed on the foot-board. If the cylinder is turned, the cord rolling more and more on it raises the foot-board, and flexes the foot. An iron stop, which falls between the teeth of the wheel of the cylinder, prevents it from returning on itself. By these means a very strong extension of the tendon may be made, and graduated at will.—*Archiv. Gen.*, Jan., 1834.

Re-union of the Extremity of a Finger which was completely cut off, by Dr. Angelo.—On the first of January, 1818, a day during which the cold was more intense than I ever felt it in our district, (at Chiavari), Emmanuel Copello, cut off at one blow with a very sharp knife, the extremity of the index finger of the left hand. The division took place transversely below the root of the nail. The child immediately took up this portion of the phalanx which fell on the ground, re-applied it to the wounded finger, and hastened off to the shop of M. Podeste, an apothecary, about, fifty paces distant from the place where the accident had just occurred, for the purpose of having it dressed. There he took off the detached portion of

the finger to shew the cut, and re-applied it immediately. At the end of about fifteen minutes I came to the shop. I examined the finger carefully, and observing that the child had not replaced the extremity of the phalanx, so as to correspond exactly with the finger, I wished to remove it a third time; but whether the coagulable lymph made it already adhere, or what is not very probable, whether this adhesion was the effect of coagulation of the blood, I found the approximation so solid, that I did not think it right again to detach the extremity of the finger by employing force; I dressed it, however, the best way I could, and finally, kept it in its place by means of two straps of diachylon, which I secured by two circular turns made with a third strap. The finger was covered with some linen soaked in balsam of Peru, and the whole enveloped in an appropriate bandage. I recommended the patient to keep his arm in a sling, and carefully to avoid striking his hand against any thing. The fifth day I removed the dressing, and I saw that the skin of the extremity was livid. I now thought that mortification was proceeding, when on raising the epidermis thus charged, I perceived beneath it the cutis red, and completely re-united. I encompassed the finger with lint, still leaving on the two straps which crossed each other at the extremity of the finger. Three days after I removed the dressing entirely, and was satisfied the re-union was complete. The mortified skin came off like the extremity of the finger of a glove: beneath the divided part of the root of the nail, we already saw the rudiments of a new nail.

The sensibility of the extremity of this finger remained for a considerable time more obtuse than that of the other fingers, and its point was not as thin as before. In the point corresponding circularly to the division, the cicatrix left around the finger a depressed line similar to what would exist, if the end of the finger had been for a long time compressed in the narrow neck of a bottle.—*Annali Universali di Med.*, Sept., 1833. *Archiv. Gen.* Jan., 1834.

On the Employment of Casts, (moules en platre), in the Cure of Fractures of the Leg, by A. L. Richter.—The knowledge of this practice, so very much used among the Arabs, was imported into Europe by the English consul at Bassora, who inserted an observation in the *Medical Commentaries*, dec. ii. vol. ix. It was not till 1828 and 1829, that the practice was adopted, and with the happiest success in the Hospital la Charité, of Berlin, according to the directions given by Dr. Kluge. At first they made use of a box large enough to contain the entire leg, the condyles of the femur being contained in it. The box was filled with sand to half its height; on the sand was placed a paste-board splint, six inches broad, and two others were sunk perpendicularly in the sand along the two edges of the first, so that the space included between the splints represented exactly the form of the cast. The splints and calf of the leg were rubbed with oil, and the foot kept in a proper position. They then poured on rapidly some liquid plaster, which became hard at the end

of half an hour, and enveloped half the thickness of the leg; then they removed the splints and the box. The latter was in consequence modelled (*modifié*); it was made twenty-two inches long and seven inches broad, with moveable sides, articulated on each other, and so that the one might be taken after the other from the hardened plaster. Two species of cast (*moule*) may be distinguished: the first, which is applicable in simple and continued fracture; the second, which is preferred in compound fractures, is formed of separate pieces. Then the foot can be placed in a fixed position, and the necessary dressings applied over the upper part of the leg.

Some precautions are necessary in the use of this apparatus.

1st. It is only after the swelling has attained its maximum, that the limb is to be surrounded with its cast, otherwise we shall content ourselves in fixing the foot.

2nd. The plaster should be perfectly white, prepared for some time, and finely powdered; without this, it develops some heat in solidifying.

3rd. The plaster, mixed with water, ought to have the consistence of cream; if it were more solid, the limb would be burned; it is well to try it first in the hollow of the hand.

4th. The extension is to be continued as far as the solidification of the plaster. This apparatus has over all others the advantage of keeping the fragments in the most exact manner, as well as preserving the extension, and it frees the patient from all the pains which are the result of unequal pressure, and from the excoriations which are its consequences. The patient (particularly if the leg is on a moveable board) avoids the annoyance of seeing his apparatus deranged, for he feels that the limb is supported exactly on all sides. At the end of a month the leg is gently raised, and the lowest part of the casts only is laid aside. It is in oblique fractures with projection of the bone under the skin, that this method renders the most essential services. The two fragments cannot then slide one upon the other, and can resist muscular contraction. These advantages are still more manifest in fractures which occur near the articulations.—*Extract from a work of Dr Richter's. Berlin, 1832. Archives Generales, Feb. 1834.*

Worms in the Blood.—History of a Case in which Animals were found in Blood drawn from the Veins of a Boy, with Remarks, by J. Stevenson Bushnan, F. L. S., Surgeon to the Dumfries Dispensary.—A boy, eight years of age, living on the banks of the Nith, the waters of which he had always drank, “was visited professionally in June last, by Mr. Bushnan of Dumfries, who found him labouring under influenza, at that time epidemic.”—“He complained of great headach, his pulse was full and strong, and 100 in the minute.” The mother of the patient, when she requested Mr. Bushnan's advice, informed him that her son had been bled, and that fifteen worms had been found in the blood drawn; one of which she produced in a cup of water. The worms were stated to have been

visible in the blood in less than a quarter of an hour after it had been drawn.

Mr. Bushnan deemed it expedient to abstract about six ounces of blood, in which he could discover nothing preternatural. He covered it with a basin in such a way as that it could not be disturbed without his knowledge.

In an hour afterwards he found five animals swimming in the serum, all most vigorous and lively. On dividing the clot, the animals made powerful efforts to disengage themselves from it. Eight were disengaged, making with the five discovered in the serum, and the fifteen obtained by the previous bleeding, twenty-eight. Specimens were sent immediately to Mr. Rhind, the naturalist, who gives the following description of them:—

“ The animals I received from you lived with me two days, in a little blood serum, when I had an opportunity of examining them most attentively. They are from about half an inch to six or eight lines in length; when dead the bodies relax and become about one inch long. They consist of an articulated body of eleven joints, of a head with rudiments of four organs, (antennæ and palpi,) with an appendage immediately below the articulation of the head, which is ciliated, and very similar to the respiratory tubes at the other extremity. The tail terminates in two tubular bodies or stigmata, having ciliated margins; they are the external respiratory organs; besides these, there are two or three bands on each side, which are mere fleshy appendages. Within the articulated body, extending on each side from the tail to the head, the respiratory organs are distinctly visible with the aid of a microscope. They consist of a continuous tubular structure, of a pale silvery colour, through which the air passes. The colour of the animals is bright-red. These animals exactly correspond in structure, colour, and size, to the larvæ of the *Tipula oleracea* which in summer is so abundantly found in ditch or river water. The eggs of these flies are very minute, and at a certain season of the year are deposited in great numbers in running water, by the *Tipula* fly, well known by its long legs and slender body. The worms cannot be mistaken for any of the Entozoa of the human body, or of other animals, because they have distinctly formed aerating organs, which intestinal worms never have been discovered to possess. They are in many points entirely different. Their red colour is a specific distinction, and not likely to be accidental, from the colour of the fluid in which they were formed, and on which they appeared to feed. They also seemed to respire equally well in the blood serum as in water, for I could distinguish a constant succession of air globules in their respiratory tubes.”

The short monograph from which the above account is extracted contains a brief view of the narrations given by various authors of similar occurrences, and a discussion of the various theories of their formation, to which, as we have not space here, we must refer the reader.

A Case of Scorching by Electric Fluid, by M. Volpeliere of Arles.—A very delicate woman, sixty-eight years of age, of a nervous, bilious temperament, while closing her window on the evening of 26th of May, 1829, during a violent thunder-storm, with torrents of rain, was struck with lightning. The electric fluid proceeded from the iron fastening of the window along her left hand and sleeve, till it reached her shoulder, whence it descended along the thorax, abdomen, groin, internal part of the thigh and leg, to the sole of the foot, keeping always on the same side. She fainted, fell backwards, and sustained a contused wound in the occipital region. After remaining an hour in this state of stupor, she with difficulty got up, and threw herself on her bed till day break, when she called her neighbours. On visiting her soon after, M. Volpeliere found her with a wandering countenance, feeble concentrated pulse, and cold skin. All the parts touched by the electric fluid were scorched. In some places the epidermis was raised, and the thigh presented an erysipelatous redness. Simple cooling dressings were applied, and a stimulant exhibited. On a second visit, the strength was a little restored. On examining the patient more minutely, it appeared that the skin had acted as the conductor of electric fluid. The sleeve of the shirt was not burned, except at the shoulder, where, at the time of the accident, the arm formed with the body an angle of 46° . She had a cloth under-vest lined with scarlet. Wherever the electric fluid had been delayed for a longer or shorter period, the scars were larger and deeper. The wrist, shoulder, upper part of the pelvis, were severely burned. The inner surface of the thigh, at first of an erysipelatous red, presented soon after the reaction an enormous wound. The leg confined by the garter did not arrest the course of the fluid. There were observed here only two holes, one below the knee, and the other above the *malleolus internus*. Reaction took place several times, but was subdued by abstinence. After the separation of the eschar, the cicatrization of this enormous wound took place; but upwards of two months after the accident, the patient died of gastro-enteritis.

Wound of both Femoral Arteries without Hemorrhage.—An artilleryman, while standing with his legs asunder near a cannon which he was preparing to sponge, was struck by a bomb, which burst, passed between his thighs, from behind forward, without fracturing either femur, and tore all the soft parts situated at the middle-third of the internal and posterior side of these members. No hemorrhage ensued, though the two femoral arteries appeared, amid the havoc of the soft parts, completely divided, and exhibiting a motion altogether erectile. The pulsations of both were very perceptible superiorly, till within three or four lines of the divided extremities, where a sort of resistance to pressure was felt, as if the orifices of the two arteries had been stopped by a foreign body. It was true torsion of the vessels. It is almost needless to add, the patient did not survive.

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PART I.
ORIGINAL COMMUNICATIONS.

ART. XVIII.—*On Rupture of the Peritoneal Coat of the Uterus.* By FRANCIS WHITE, M.R.C.S., Surgeon of St. Anne's Dispensary, &c. &c.

(Read before the Surgical Society of Ireland.)

THE following case, which occurred some time back, I beg leave to offer to the notice of the Society; it is different in many particulars from the only two cases on record of rupture of the peritoneal coat of the uterus, and therefore will, I trust, not be found altogether devoid of interest.

Mrs. W——, a highly respectable lady of this city, aged 32, well formed, married fifteen years, the mother of eight living children, had nearly gone to the full period of utero-gestation of her ninth child, when on the 10th December, 1824, she met with some fright that caused her to turn round quickly; she was at the same moment seized with pain in the lower part of her back, which extended round to the abdomen, at-

tended with a sense of faintness and great palpitation of the heart. She recovered soon from the immediate effects of this shock, and being of a very cheerful disposition, and of an active turn of mind, no further apprehensions were entertained either by herself or those about her, although it was observed she looked paler, and appeared more languid than usual. However, she attended to her domestic affairs until the morning of the 18th, when going up stairs she was attacked with darting pains in the lower abdominal region, attended with a peculiar sensation which she could not well describe; she became agitated, pale, and ghastly. A late eminent accoucheur was immediately sent for, who found her labouring under great difficulty of breathing, threatening suffocation, pain of her heart, pulse quick and fluttering; there was no appearance or symptom of her labour coming on, and seeing her situation becoming more alarming, Dr. Cheyne was called in in consultation. About nine o'clock, p. m., Mrs. W—— was seized with labour, and after a few feeble uterine pains, she was delivered of a full grown still born male child; but in less than three quarters of an hour she gradually sunk and expired.

In the *post mortem* examination which took place in two days after, and in which I was assisted by Mr. Colles, the Senior Professor of Surgery, the following were the appearances which we observed and noted:—

Cavity of the Thorax.—Considerable serous effusion into the right side; on the left, there were adhesions of the pleuræ, and some effusion; the structure of the lungs healthy; the heart empty, its walls flaccid; valves of the arteries without diseased appearances.

Abdominal Cavity.—On opening the abdomen, a large quantity of fluid blood was found in the vicinity of the uterus, the broad ligaments of which were injected with blood; the uterus had not contracted; the right ovary was much enlarged, and contained two hydatids of considerable size; on the anterior surface of the uterus were two long tears or lacerations, and one of smaller size

through the peritoneal coat, and also through a few superficial fibres of the uterus, from which the blood had issued. All the other parts, both of the pelvis and abdominal cavity, were perfectly sound, and on opening the cavity of the vagina and uterus, nothing was observed except what is usual after parturition.

Such were the appearances which presented themselves to our view. The heart had been emptied of its last drop, and the abdominal cavity filled with almost the entire blood of the body, which had flowed through those three superficial rents.

From the detail of the above untoward and melancholy case, a few observations may be deemed not out of place or inappropriate, and therefore I do not hesitate in submitting them to your impartial consideration.

It will be perceived that the above case was from the commencement involved in obscurity, that it was not attended with the usual characteristic marks of lacerated uterus or injuries of its appendages. There will be found some striking differences between it and the two cases already alluded to, and which might at first view be considered as bearing a strong resemblance to it.

The first case is by Dr. C. M. Clarke, in the third volume of *Transactions for the Improvement of Medical and Surgical Knowledge*.

“A woman between the age of twenty and thirty was taken ill of her first child at 8 o'clock, A. M., attended by a midwife. She went on gradually in regular labour for about two hours, when she was suddenly seized with pain in the abdomen and nausea; great irritability succeeded, with faintness and excessive restlessness, and at half past 10 o'clock, P. M., she died undelivered, immediately after which a dead child and the placenta were extracted.

“On examination there were no morbid appearances to be found in the abdominal cavity, or any of its viscera. The uterus somewhat contracted. On viewing its posterior surface, about an ounce of blood was observed lying in the fold

of the peritoneum, where between forty and fifty transverse lacerations were discovered, none of which were in depth more than the twentieth part of an inch, many of them appearing merely fissures, and varying considerably in length from the fourth part of an inch to two inches; they occupied the greater part of the posterior portion of the uterus, and were thinly covered with flakes of coagulated blood. The muscular part of the organ was perfectly sound."

In this case it will be remarked that those alarming symptoms with which the patient was seized, occurred two hours after regular labour had taken place, that she died undelivered, and that the appearances on dissection, discovered those slight lacerations in the peritoneal coat merely, the muscular structure not being in the least implicated, and that about an ounce of blood was effused into the peritoneal fold; upon the whole, it is difficult to account for this woman's death, and Dr. Clarke does not account for it satisfactorily, and appears to incline more to the opinion of its being the result of disease, than as the effect of an accident occurring during labour, or being immediately consequent upon it.

The second case is in Dr. Ramsbotham's work on Midwifery, page 409. "A lady who had the previous evening been delivered of her seventh child, after a tedious labour; she gradually sunk, and died somewhat suddenly the following morning. No particular occurrence to excite alarm had happened during labour, nor was there any external flooding. On dividing the abdominal parietes a considerable quantity of blood was observed in the cavity of the abdomen; the viscera generally were healthy; on bringing forwards the uterus, which was well contracted for the time, a rent of several inches in length was discovered in the peritoneal coat, on its posterior surface, extending nearly to the insertion of the left broad ligament, in which the fleshy structure of the uterus did not seem to be engaged."

The fatal result of this case can be fairly attributed to the

tedious and difficult labour, and undue degree of pressure of the distended uterus against some portion of the lumbar vertebræ or angle of the sacrum, would account for this extensive laceration in the peritoneal coat.

Respecting this case, I shall merely observe, that it bears a stronger analogy to the one which I have recorded than that of Dr. Clarke; however, there may be found some shades of difference; first, the symptoms which indicated danger did not set in until after the patient was delivered, and secondly, the appearances on dissection presented this material difference, that the injury was confined to the peritoneal covering.

Having thus adverted to the only two cases which I have been able to find bearing upon the present subject, I shall now briefly comment upon the case immediately under our consideration. First, then, the injury which led to her death was caused by the exertion occasioned by the fright which occurred some time previous to delivery, and totally unconnected with it; and secondly, the symptoms denoting such, were of so equivocal a nature, as to be calculated to deceive the most experienced practitioner. For though it was almost obvious she was labouring under internal hemorrhage, yet there were no positive or conclusive symptoms to indicate its having an uterine origin; and thirdly, as I have before stated, the lacerations penetrated into the muscular structure of the uterus, and, it will be observed, were confined to the anterior portion of the organ.

It is a matter worthy of consideration, whether, if the real nature of the injury had been known, any chance of saving the patient would have been afforded, by inducing premature labour and an early extraction of the child; the consequence of which might have been a contraction of the uterus, and, in all probability, a cessation of the hemorrhage.

In this view I am supported by Dr. Charles Johnson, our Professor of Midwifery, whose experience and scientific acquirements in this particular branch of the profession entitle his opinion to the highest consideration; and who states in a letter

to me, on perusal of this case, that, "Rupturing the membranes, by allowing the uterus to contract, and thus diminishing the calibre of the vessels, might, in your case, have checked the hemorrhage; but most probably would not have altered the event of the case; for we find by Dr. Clarke's case, that this injury of the uterus may prove fatal without hemorrhage to any extent. The rupture of the membranes, would likewise have been the means of expediting the labour; and we know that the chances of recovering in all cases of ruptured uterus, are much lessened if the woman is not immediately delivered."

In conclusion, I have to observe, that there is no subject connected with the practice of surgery and medicine, that demands so careful an investigation as the causes of sudden death which occasionally occur during pregnancy, and which, in my opinion, have been too much overlooked by practitioners. It admits of little doubt, but by attention to this very important subject, the profession may be enabled to ascertain, by post mortem examinations, the true nature of such accidents, and thereby arrive at a more accurate knowledge of the mode of treatment. Even if we cannot deduce any practical inference from the case under consideration, it may be valuable, by throwing additional light on a subject hitherto involved in obscurity. It is then with these views, that I have brought forward the present case, in order to encourage the junior portion of our profession, to pay that attention to the subject which it so particularly merits.

The following note from Dr. Montgomery throws additional light upon this subject:—

"18, Molesworth-street,

"February 20, 1834.

"MY DEAR SIR,

"While listening to the detail of your very interesting paper on peritoneal rupture of the uterus, read before the Surgical Society, it occurred to me that I had met with an account of another case, besides those to which you referred,

and on turning to the notes of my lecture on the subject, I find the following case quoted: 'A patient during labour, suddenly complained of unusual pain and distress, and presented the most alarming symptoms, without any evident cause.' She was, however, safely delivered by the natural efforts of the uterus, but fell into incoherence, jactitation came on, and she died within six hours after delivery.

"The body was examined next day, and a large quantity of blood was found effused into the cavity of the abdomen. The uterus was firmly contracted, and posteriorly, near its fundus, was found ruptured to some considerable extent. The surface of the rent was covered with portions of coagulum: it occupied a space perhaps as large as a crown piece, but of irregular margins, and surrounded by a reddish stain, giving it, at first view, the appearance of having resulted from ulceration. Near this large breach were *three or four smaller cracks* in its substance. Upon cutting into the cavity of the uterus, it was found that the rupture had not extended into it, the lining membrane being entire throughout, and a considerable portion of muscular substance intervened between it and the external rent, which perhaps had not penetrated more than two-thirds through it. The uterus seemed sound in all other parts of its structure.'

"You will find this account in the London Medical Gazette for August, 1832, p. 630, by Mr. Chatto, and it might be satisfactory to you to add it as an additional reference connected with your very important case, and no less judicious observations thereon.

"I remain, dear Sir,

"Very truly your's,

"W. F. MONTGOMERY."

ART. XIX.—*On the Cause of the Pulse being affected by the Position of the Body.* By TRAVERS R. BLACKLEY, A. B., Member of the Royal College of Surgeons in Ireland.

(Read before the Surgical Society of Ireland.)

UNTIL the appearance of Thompson's work on Inflammation, the attention of the medical world does not seem to have been drawn to the fact, that the position of the body has considerable influence in modifying the action of the heart. In speaking of the effects of the cold bath in lowering the pulse, he makes a few observations on the subject :

"The pulse," he says, "even in a state of health, is more frequent by from twelve to twenty beats in the minute in the perpendicular than in the horizontal position. This difference," he adds, "was pointed out to me for the first time many years since by Doctor Mac Donnell, of Belfast.

From this time little seems to have been done either by physiologists or pathologists to account for these phenomena. I am not aware of any work purporting to have this for its object, and for many years we have contented ourselves with the very insufficient explanation of those varieties being caused by *muscular exertion*. So little interest, indeed, had the subject created of late years, in relation to either physiology or therapeutics, that I believe I would be justified in asserting that the majority of medical men, previous to 1830, were either ignorant of the fact, or had ceased to regard it as of any moment whatsoever. At this time public attention was revived by an able and valuable paper from the pen of Dr. Graves, "On the Effects produced by Posture on the Frequency and Character of the Pulse in Health and in Disease," which appeared in the fifth volume of the Dublin Hospital Reports, 1830, and which, for closeness of observation and accuracy of experiment, is no less interesting to the physiologist than to the medical practitioner. "In healthy persons," he observes, "the pulse in the erect posture is more frequent than in the horizontal by from six to fif-

teen beats in the minute. If the pulse is but sixty, the difference is generally not more than six or eight, and this difference increases with the frequency of the pulse at the time of the experiment; thus, if it has been raised to 90 or 100 by moderate exercise, it is not unusual to find the difference twenty or thirty. I need scarcely add," says Dr. Graves in conclusion, "that I cannot advance even a plausible conjecture concerning the reason why a change of position should so affect the frequency of the pulse."

These last words stimulated my curiosity, and led me to the following conclusions in explanation of the causes of the heart's action being influenced by the position of the body :—

I believe it will be readily conceded, that the action of the heart in a strong and healthy individual, while in a state of rest, is uniform and equal, that it is possessed of a power sufficient to expel a certain quantity of blood at each contraction of the left ventricle, which power is necessary to overcome the obstacles presented to the egress of the blood.

Let us suppose, for instance, that the heart of a healthy man in the erect posture beats sixty times a minute, and at each beat expels one ounce of blood, sixty ounces per minute will be of course expelled; but if the *power* of the heart be increased or diminished, we must expect a corresponding alteration in the number of beats. Thus, if the power be *increased* one-tenth, it will require but fifty-four beats to expel sixty ounces in a minute; but if it be diminished one-tenth, it will require sixty-six beats.

To this variation of power in the heart are the phenomena of which we speak attributable, or rather I should say, *the variation of resistance to the heart*, which comes to the same thing. A question may here be asked, namely, "can you prove that the heart discharges a greater quantity of blood at one time than another, or that the left ventricle does not at each contraction expel the entire of its contents?"

I am fully aware that the general opinion is opposed to mine ; indeed, actual experiment would seem to be against me, for it has been affirmed, that on inserting the finger into the left ventricle of a recent heart, it contracts through its whole extent ; nevertheless, I think I can prove the reverse, from the anatomy and mechanical construction of the heart itself.

1st. There are no muscular fibres going from the substance of the left ventricle to be inserted into the opposite edges of the valves, or above them into the aorta ; therefore the space immediately inferior to the valves cannot be compressed.

2dly. We never find the left ventricle perfectly closed after death, even though it should be at the time that rigidity of the muscular fibre prevails.

3dly. If the valves had no support, as by a certain quantity of blood under them, to act as a counter pressure, they would be liable to injury from the superincumbent mass of blood in the aorta.

4thly. A vacuum being formed by the expansion of the ventricle, the valves would be drawn downwards with considerable force to fill it up, and thus an additional weight be unnecessarily imposed on them.

Lastly. In a very rapid pulse, say 160, we can scarcely conceive the sides of the left ventricle to be closely approximated through their whole extent at each pulsation, and separated again, the usual quantity of blood as in health being expelled at each contraction. But we can readily admit that such a number of palpitations may be effected, the ventricle contracting but slightly at each beat ; or a very small quantity of blood may be contained in the ventricle at each contraction, and thus supersede the necessity of the ventricle being much dilated ; and we may observe, that in these cases where the pulse is so frequent as I have mentioned, it is never full and strong, but on the contrary, weak and thready, evidently proving that a very small quantity of blood indeed is forwarded at each contraction of the ventricle.

To resume. Let us consider the relative force of resistance to the heart's action in the erect and horizontal positions of the body. In the former we have the column of blood in the arch of the aorta assisted by that in the carotids pressing on the semi-lunar valves, and opposing the egress of blood from the left ventricle. Next, we find that the arteries being all full, a considerable *vis a tergo* is required to force on the blood which they contain, especially through the carotids, where it must be driven upwards. But by far the greatest obstacle to the action of the left ventricle, and that which is the chief cause of the non-permanency of the pulse, is presented by the veins; if the arteries require the *vis a tergo*, the veins require it in a much greater degree, not only from the nature of their structure, which is inelastic, but that their contents, contrary to the laws of gravitation, must for the most part be forced directly upwards to the heart.

In the horizontal position those obstacles are lessened or removed; the blood in the carotids and arch of the aorta does not press with such force upon the valves, but chiefly the veins, namely, all those below the heart, being placed in the most favourable position for *spontaneously* returning their contents, remove an immense obstacle to the egress of blood from the left ventricle. Hence it follows, that less resistance being opposed to the heart in the horizontal position, and the same power exerted, a greater quantity of blood is propelled at a time, and consequently the number of pulsations necessary to transmit the same quantity in a given time in the erect posture diminished.

The frequency of pulsation then is in a direct ratio to the obstacles presented to the heart's action, whether those be mechanical or arising from debility of the heart itself. On the supposition of the correctness of this view, can be explained, I think, all the phenomena so closely observed by Dr. Graves.

“As the muscular exertion necessary to keep the body in

the erect posture," observes Dr. Graves, "might be considered as the cause of this greater frequency; it became necessary to contrive means of placing the body in any desired position, without the necessity of muscular exertion on the part of the subject of the experiment. This was effected, and it was found that when the posture was changed by means of such a contrivance, the difference between the frequency in the horizontal and erect postures was not less than when muscular exertion was used." The theory of muscular exertion then being the cause, is set at rest for ever. "I now anticipated that if the body was placed with the head down and feet up, a still further retardation of the pulse would be produced. It was, indeed, natural to suppose from the preceding experiment, that posture alone was the cause of the retardation observed in the body when placed horizontally, and, consequently, that this effect would be augmented on still more depressing the head, and that the maximum of retardation would occur in the inverted position. Here, however, as it not unfrequently happens, preconceived ideas were not found to accord with experiment, and no further retardation was thus effected; neither, on the other hand, was it accelerated beyond the number observed in the horizontal position."

I quote Dr. Graves thus fully, and am anxious his experiments should be borne in mind, as the observations they suggest will serve materially to elucidate the grounds by which my theory is to be supported.

Thus, in respect of the foregoing observations, although in the inverted position there certainly is a greater facility for the return of the blood contained in the veins below the diaphragm to the heart, yet a new obstacle is offered to the action of the left ventricle in the relative position of the arteries. The blood in the aorta, iliac and femoral arteries, &c. &c., must in this position be forced upwards, instead of gravitating to a certain extent downwards, as they do in the erect posture, and the blood in the veins of the head, neck, &c. will require a greater

vis a tergo to be forced upwards to the heart. Hence I think we might readily deduce a rule to ascertain the relative force of opposition in the veins and arteries to the action of the heart in the various positions of the body. Thus, if in the erect posture, the most favourable for the transmission of blood in the arteries, and most unfavourable for the veins, the heart contract eighty times a minute, and in the horizontal or inverted position, the most favourable for the return of the venous blood, and unfavourable for the arterial, it beat only sixty times, I conclude that the resistance opposed by the veins is one-fourth greater than that offered by the arteries. The easiest posture, then, in which the heart's action can be carried on, is that in which the arteries and veins are as little antagonized as possible; namely, the horizontal. "It is very singular," says Dr. Graves, "that a posture so unnatural as the inverted should produce no effect on the frequency of the pulse as compared with the horizontal, while a change from the latter to the erect, both natural postures, is attended with so great an acceleration."

The reason is obvious; the opposition to the heart's action is the same in the two positions, the horizontal and inverted, the increased resistance in the arteries in the latter position being exactly balanced by the increased facility for the return of the venous blood, whilst in the erect posture the resistance of the veins is much increased over that of the arteries, and the general opposition to the heart much greater than in either of the former positions. "In the inverted position," he continues, "although the *frequency* of the pulse is not altered, its strength is diminished, and often very considerably; it is not unusual too for it to become irregular, a fact that may be explained by the greater weight of the blood pressing back on the aortic valves, and then necessarily opposing an unusual impediment to its egress from the left ventricle."

I would suggest that the diminution of strength of pulse, observed in the inverted position, may in some manner be attributed to the pressure on the brain, or to the very unusual posi-

tion of the heart, which is now turned upside-down, and its strength and mobility thus lessened and confined. The same causes may exercise their influence in causing *irregularity*, but this latter is, I think, more directly referrible to the superabundant quantity of blood, which in this posture is thrown into the right side of the heart, and which thus prevents the *equilibrium* being maintained between the two ventricles, and is not to be attributed to pressure on the valves, as there is at such a time much less pressure, than in the erect position when no such irregularity exists.

"The pulse," he adds, "is also evidently stronger in the horizontal than in the erect posture, consequently its maximum of strength, and minimum of frequency, are attained together."

I would here remark, that the increased strength and (we may add) fulness of pulse observed in the horizontal position compared with the erect, depends on the resistance to the heart being diminished in the former, and a greater quantity of blood consequently expelled at each contraction, the quantity of blood being thus increased the number of pulsations required to transmit a given quantity is diminished, as I have already explained, whereas if the quantity of blood was diminished the number of pulsations would be increased. Mr. Adams, in his paper on diseases of the heart,* gives a case in which he expresses himself in harmony with this opinion.

"In consequence," he says, "of the shortening of the valve it imperfectly covered the auriculo-ventricular opening, and too readily allowed of a reflux of blood into the left auricle during the contraction of the ventricle. Hence the effort of the heart, instead of being, as it is in the natural state, expended in propelling the blood onwards through the aorta, was partly lost, because of the imperfect state of the valve admitting a regurgitation of some of the blood which was destined to pass into the aorta. The heart was obliged therefore to reiterate its beats,

* Dublin Hospital Reports, vol. iv.

to compensate by its quickness for that small quantity of blood it was incapable of furnishing at one contraction through the aorta."

Dr. Graves now proceeds to make some observations which are highly interesting, and which serve to prove *pathologically*, (should any doubt remain,) the correctness of the theory I have advanced.

"In all other diseases in which I have investigated this subject," he says, "I have found a difference between the frequency of the pulse in the erect, sitting, and horizontal postures, but in six cases of hypertrophy with dilatation of the heart, no such difference was perceptible, although all these patients at the time of my making the experiment were in a debilitated state, which is that in which the changes induced by position are the most remarkable.

"In those cases the hypertrophy and dilatation were very great, and in five of them certainly, (examined after death,) and in the sixth probably, the left ventricle was involved in the disease, and I am inclined to think that this permanency of the pulse in all positions of the body, will be only found to exist in such cases, and not in those where the hypertrophy and dilatation are less considerable, and consequently the diagnosis more obscure. This circumstance may, it is true, detract from the value of the observation, so far as regards diagnosis; but certainly does not diminish its physiological interest.

"I may observe too, that should future observations prove that hypertrophy of the heart is not always attended by this permanency of the pulse,* and I believe it is not, yet its occurrence in so many cases of that affection is nevertheless an interesting fact."

* From a conversation I had with Dr. Graves, a few days ago, it appears that since the above was written, now four years, he has in his very extensive practice uniformly observed that permanency of pulse is caused by hypertrophy of the left ventricle.

Nothing can be more satisfactory than those remarks, they prove that the left ventricle being possessed of a certain additional power, namely, sufficient to overcome the resistance offered to the heart, in all positions of the body, that permanency of the pulse is the consequence, and therefore I think it follows that the non-permanency of pulse, or the variation of it, depends on the *inability* of the heart to overcome the resistance opposed to it, with equal facility in all positions.

The state of the valves, however, as Dr. Graves very accurately remarks, must have considerable influence in modifying the action of the heart, for on their integrity, as we may readily perceive, depends in a great measure the relative resistance opposed to the left ventricle, yet we may conceive the same effect "permanency of pulse," to be produced in two very different states of disease, namely, hypertrophy *with* and *without* valvular disease. Thus hypertrophy to a certain extent, assisted by valves in a healthy condition, may cause permanency of pulse, while hypertrophy to a much greater degree, even though the valves be obliterated, may give rise to the same effect, the increased muscularity and consequently *power* in the latter case, being equal to the assistance afforded by healthy valves in the former.

A question arises. If increased muscularity or hypertrophy be acquired by the heart, in consequence of the obliteration of the valves, what would be the result, if the valves became so strengthened as to remove in a great measure the pressure of blood from the left ventricle? I reply, diminished muscularity or atrophy. This I am fortunately enabled to prove by reference to Mr. Adams' treatise on diseases of the heart,* in which the following case occurs, one, the most interesting and extraordinary perhaps that has ever attended pathological research, namely, the change of the muscular structure of the heart into fat.

* Op. cit.

“The right auricle was much dilated; the right ventricle externally presented no appearance whatever of muscular fibres, it seemed composed of fat throughout almost its whole substance. The left ventricle was very thin, and its whole surface was covered with a layer of fat, beneath this the muscular structure was not a line in thickness.”

This I think will be readily admitted to have been a completely atonic condition of the organ, but let us inquire the cause. “Indeed the left ventricle was so weak, and its parietes so reduced, that, at first sight, it created our surprise that it was at all capable of carrying on the circulation, which was not a little increased when on a close examination it was discovered, that the valves of the aorta had become, from cartilaginous and earthy depositions, so rigid as to retain water, whether poured upon them from the artery or the ventricle, the contact of their edges was preserved in such a manner, that it required that a fluid should be injected from the heart with some little force to render this arterial opening pervious.” This case speaks volumes, it is an additional proof of the immense influence which the valves exercise over the action of the heart, and it serves to place in strongest contrast, two opposite states of disease induced by the condition of the valves, namely, *atrophy*, as in this case, caused by the unusual strength of the valves, and the support afforded by them to the column of blood in the arteries, superseding the necessity of the usual muscularity of the ventricle, compared with hypertrophy induced by obliteration of the valves, and the consequent necessity of *increased* muscularity, to overcome the *additional* obstacles consequently presented to the egress of the blood. This, at first sight, may appear contradictory, as the state of the valves presented an additional obstacle in itself, but let us recollect, that the heart, like other organs, may accommodate itself to changes, when effected *gradually*, that would cause instant death if suddenly induced.

While speaking of the valves, and the influence they exert, it may not be uninteresting to mention that Haller was fully

aware of the effect produced by them, when in a diseased state, on the heart, though not perhaps quite clear as to the cause. In his work, *De Partium Corporis Humani Fabrica et Functionibus*, he gives several instances, among others the following: "*Valvulae aortae contractae ut sanguis in cor rediret, hinc ventriculus sinister, dextro amplior.*" Again, "*Cum eadem ostiola ossea forent, et sanguinem retrogradum in cor admitterent, cor sinistrum amplissimum.*"

Very many practical results and inquiries are deducible from the foregoing observations, not the least useful of which would be the relative degree of hypertrophy induced by the various morbid conditions of the valves; and also the propriety of lessening the mass of the circulation, and removing the resistance to the heart by observing the horizontal posture, in hypertrophy as a primary and uncomplicated disease.

It has been observed, that the greatest variety in the frequency of the pulse on change of posture occurs in persons labouring under fever, or debilitated in consequence of that or any other cause, amounting in some cases to so great a difference as even fifty between the erect and horizontal positions, and this we might expect on considering the structure of which the heart is composed, and the purposes it serves.

It is a muscular organ, and participates with the other muscles of the body in the general debility of the frame in sickness; not being able to overcome the obstacles presented to the egress of the blood in the erect posture, in as few pulsations as in the horizontal, it is forced to contract the more frequently in order to transmit the same quantity of blood in a given time as in the latter position, at least it makes an effort to do so, and this greater variation is therefore a good diagnostic sign of the general debility of the patient and *vice versa*; of course when the patient lies down the pulse falls to its former standard, the obstacle which caused its increased action being removed, in

other words, the veins now empty themselves *spontaneously*, instead of requiring the *vis a tergo* from the heart to propel the blood through them, consequently the resistance to the left ventricle is lessened. It sometimes, however, happens, that the pulse does *not* fall to its former standard on the person lying down, and the cause is interesting.

On assuming the erect posture from the horizontal, (when much debilitated,) the heart is affected in two ways, first, by the irritation of an unusual stimulus, secondly, the actual obstacle to the return of the venous blood; where the weakness is not to a great extent, it becomes accustomed to the irritation, and consequently after some minutes falls a few beats, but still remains more frequent than in the horizontal posture, being affected by the latter obstacle, the non-return of the venous blood. It may, however, occur, that the irritation once induced may continue, and the increased action of the heart be thus kept up, even after the patient lies down, and this is a good diagnostic sign, as it indicates excessive weakness or irritability: It has been observed by Dr. Graves, that in some persons the increase of frequency in the pulse is greater between the horizontal and sitting postures, than between the latter and the erect, while in others the contrary takes place, so that generally the frequency in the sitting posture may be taken as a mean; this is perfectly correct, and agrees fully with the theory I have advanced, for the femoral vein in the sitting posture is horizontal, and the weight of the column of blood from the feet to the head is broken, consequently the resistance to the left ventricle much diminished. In like manner we may find, that if we now raise the legs to a level with the thighs, we shall have a further retardation of the pulse by from two to four beats; this, however, will depend much on the age and strength of the individual, and on the healthy condition of the lower extremities in general, and the veins of those parts in particular; thus, where the limbs are anasarcons, or the veins varicose, we may expect a greater modification of the pulse in this position than where the legs are

pending, while in a strong and healthy young person, little or no difference is at times to be observed whether the legs are perpendicular or horizontal.

ART. XX.—*Some Considerations on Scrofula, and the various means of Cure employed at the Children's Hospital, Paris, more particularly on the Effects of Iodine in the Treatment of this Disease.* By JAMES EAGER, M. D., and House Surgeon of the Paris Hospitals.

If there exist diseases which require strict attention, as well from their frequency and importance as the various forms under which they present themselves to our view, the affection commonly denominated scrofula, may be said to belong to this class. In the following pages, I purpose to examine the subject as fully as the limits of a periodical will admit, under a conviction that any reflections with a view to the prevention or cure of this dreadful malady (too frequently a cause of despair to families) cannot fail to be advantageous to the public. The task I propose to myself is difficult, however, being in possession of some important data regarding the etiology and treatment of this affection, it would be a great omission not to make them known.

Ancient writers have had a very erroneous idea of this disease, in considering it confined to the surface of the body. This circumstance is attributable to their not having made post mortem examinations, and consequently to a want of an opportunity of comparing the symptoms with the lesions to which they belonged. We are indebted to Fernel, Plater, and particularly the illustrious Morgagni, for our present knowledge of its nature. These men, with the light of pathological anatomy, (of which they may be considered the founders,) have established the necessity of comparing the morbid symptoms observed during life, with the disorganized appearances found after death; and

from their researches, since frequently repeated by others, we are bound to conclude, that, in considering this disease solely on the surface of the body, we can have but a very inadequate idea of its extent and importance. In effect, scrofula in its progress, not only attacks the lymphatic glands of the surface, but also the most important viscera and even tissues, such as the bones, which differ widely in structure from these glands.

When the causes of a disease are obscure, it is generally chosen as an arena for the discussion of the strongest and most opposite opinions. This has been the case in the present instance. Some have ascribed scrofula to the existence of a virus, others to the presence of acids and alkalies in the economy, and probably the greater number, up to the present century, believed it hereditary, viz. that the *germ* of it was transmitted from one generation to another. The natural conclusion from all these suppositions is, that the disease bears the seal of antiquity, and is very hard to be cured; hence the necessity of well defining its nature, of unravelling its real causes, of marking its progress, its effects, the means calculated to prevent its development, and, when it exists, the remedies to be opposed to it.

ON ITS NATURE.

The following *exposé* gives the most general idea of the nature of this affection. It consists of tumours of a round form, varying in size from a millet seed to a turkey egg, sometimes encysted, more frequently otherwise, single or numerous, isolated or in a mass. These tumours are composed of the lymphatic glands, enlarged and indurated, or more frequently having undergone the tubercular disorganization. In the latter case, the substance which forms them is opaque, of a pale colour, at first hard, like concrete albumen, (*période de étudite*;) afterwards it becomes friable, soft, and finally liquid, like pus, (*période de ramollissement*.) In it we find no trace of organization. It may exist in the tissues in an infiltrated state, such as in the bones and around the articulations; and although the

character of the affection does not change, the name of tubercle is no longer assigned to it in this form.

The lungs, the mesenteric and cervical glands, the prostate, spleen, ovaries, brain, cerebellum, spinal marrow, liver, and bones, are the organs most frequently attacked, and in the order just laid down.

It is obvious that this description applies especially to what is termed tubercle; in scrofula, however, there are other appearances which are better given with the symptoms of the disease, particularly as the anatomical examination throws no light on their nature; nevertheless, as scrofula, when confirmed, rarely exists without tubercle, it becomes necessary to notice the latter; at the same time avoiding to infer the identity of these affections, from their co-existence, a conclusion which is inadmissible in the actual state of our knowledge of the subject.

CAUSES OF SCROFULA.

In a scrofulous patient we have two distinct things to observe, viz. the predisposition and the actual disease. Although authors admit that individuals of all temperaments may contract this affection, yet they seem to think it has a kind of predilection for lymphatic constitutions; a little reflection cannot fail to show the extreme difficulty, if not impossibility, of establishing this fact, admitting for a moment the existence of a lymphatic temperament, (which at most can be but a negative one, and consequently a mere creation of the mind,) it would be necessary to study the effects of scrofula on equal numbers of this and the other temperaments, in order to arrive at any thing like conclusive evidence. Were I even to abstract from these remarks the very great difficulties which oppose the attainment of this object, the numerical predominance in populations of what are called lymphatic habits suffices to explain the frequency with which these temperaments (when under the morbid influence of the causes of scrofula) become affected, and consequently precludes all possible necessity of attributing the fact to some peculiarity of texture.

The following are the external marks of this predisposition, a fine white skin, delicate features, a florid complexion, an elevated and thickened upper lip, a precocious intelligence, a large head, flat chest, a large lower jaw, dark colored teeth, which soon become carious, a large belly, flesh flabby and soft, the extremities of the bones thick and large, fair hair. This last character offers very many exceptions. In my scrofula wards, there are 56 boys and 74 girls, and of this number there are as many with dark as fair hair.

These predisposing signs do not always exist, but when an individual is so affected it is remarkable that he seldom escapes the disease, unless by an unceasing attention, from the very commencement, to the hygienic precautions which counteract this disposition.

EFFICIENT CAUSES OF SCROFULA.

Many causes are said to originate scrofula. The continued action of a cold moist air is with many authors the principal one. I have abundant reasons to think that the absence of the direct solar rays, and a long sojourn in a confined atmosphere, contribute more than any other towards the development of scrofula.

All physiologists are aware that these circumstances I mention are calculated to give a predominance to the white liquids of the body, to excite the action of the lymphatic system, and to impart to the organization of animals and vegetables that remarkable blanched appearance called "*etiolement*."

I have questioned very attentively the parents of 74 scrofulous children, and learned that of this number 56 lived in low houses with very small windows, on the ground floor, where the direct rays of the sun never penetrated, and in which, though very small, five or six persons slept. The remaining 18 belonged to individuals in better circumstances, and although consequently better nourished, were in every other respect in the same condition as the 56. The greater part belonged to the lower classes. They were subjected to great privations, and live in

rooms exposed to the north. These circumstances augmented the predisposition, and frequently brought on the disease in cases where this predisposition did not exist.

Inflammation is considered a frequent cause of scrofula in all temperaments. All substances susceptible of becoming acid on the stomach are said to induce this disease. An illustrious professor of this faculty thinks inflammation acts the principal and exclusive part in its production. It is true inflammation has been frequently found to coexist with tubercles. Tubercles seem sometimes produced in the brain by encephalitis; bronchitis, pneumonia, and pleuritis have often existed with those of the lungs. Those of the mesentery are almost always an effect of gastro-intestinal inflammation, those of the liver, that of a chronic inflammation of the pyloric extremity of the stomach and duodenum, and in some cases have even succeeded hepatitis.

These facts, although remarkable, are far from conclusive, because inflammation does not give rise to tubercles without the cooperation of other causes. Do we not often find inflammations terminate without tubercles or glandular indurations in warm seasons, whereas the contrary is the case in opposite circumstances. We also find persons kept for a long time under the influence of cold and moisture, and who nevertheless do not become tubercular nor scrofulous, whilst others in a similar situation are speedily affected.

It is worthy of remark, that the persons thus affected are most commonly women and children, in whom the lymphatic temperament predominates. I have five observations which offer exceptions to this fact, they are of persons of a decidedly sanguine temperament, in whom, by auscultation and percussion, we could not detect the slightest sign of tubercles, but who, nevertheless, in the course of traumatic fever, were seized with pneumonia; and this affection was speedily followed by the most characteristic symptoms of pulmonary phthisis. M. Andral mentions in his work cases of mesenteric tubercles in

white in which, after death, all trace of phlogosis was found. Here I anticipate an objection. In the first case tubercles (miliary) might have existed in the lungs in a latent state; and all stethoscopists know that in these circumstances it is extremely difficult, without the help of precursory symptoms, to detect them. With regard to the second case, were we to judge from analogy, we might not do infer the absolute non-existence of inflammation, from the absence of proofs *à posteriori*; as we are well aware that it is only when inflammation exists to a certain degree that it leaves traces after death. There are erysipellatous affections of the intestines as of the skin, and in the latter, during syncope, the result of bleeding, and after death, we often find no traces of irritations that visibly existed previously to these changes; moreover, the slightest irritation in one system suffices to bring on a sympathetic one in another, which is disposed to it. This we see every day. The use of farinaceous food is said to determine tabes mesenterica and scrophula, and this same cause may (according to some authors) bring on phthisis. It is very remarkable that herbivorous animals contract phthisis very frequently, whereas carnivorous ones are very seldom affected. Butchers are also seldom attacked by it. Are we to ascribe this exemption to their constant sojourn in an atmosphere charged with animal emanations? This is probable, and the more so, as we are aware that good animal food is amongst the most effectual accessory means of cure in scrophula. It may be of use to persons threatened with tubercular phthisis, to give a preference to animal food. This is a mere supposition, although I know of no facts to support it, yet it appears to merit a trial. I have been induced to advert to phthisis in this article from the frequent occurrence of this disease in scrophulous patients. In a word, if you examine a great many individuals affected with scrophula, you will certainly find that when the disease is intense, and of long duration, the lungs are almost constantly affected with tubercles. This is so true that you may conclude *a priori*, when scrophula exists in a subject in its ap-

get, that the lungs or mesenteric glands are the seat of tubercular masses. This latter fact is important as regards tubercles mesenterica, particularly as there are two states of this affection, one in which the glands are simply indurated and a little enlarged, and a second in which they have undergone the tubercular disorganisation. The first may be cured by judicious treatment, but the second is incurable from the very fact that tubercles are not resorbed; and, as from their position in the mesentery, they cannot be eliminated by the suppurative process, they keep up a constant and destructive irritation which always terminates by death. The reader, I presume, after having considered the greater number of the causes here laid down, has come to a conclusion, that from their contradictory results they can, at most, be but accessory in their mode of action. This view appears to me very accurate. In the first place it is evident, that in scrofula, all the economy seems to have undergone a fundamental and profound modification from the influence of causes the action of which has continued for a long time. Now of all causes, none is more general in its action, none that affects both rich and poor more frequently than *poor, confined air*. It is an established fact, that pure air is indispensable to the healthy state of the functions, and that when it does not contain its due proportion of oxygen, it modifies the *respiration* in a singular manner. In the first instance, through the medium of the lungs it impregnates the blood with a principle that pervades the body, giving health and life to the organs, by supplying them with arterial blood; whereas in the second, the blood not having received a necessary quantity of oxygen, passes into the system with its venous qualities, and by producing an unhealthy action in the processes of assimilation and decomposition, blunts the sensibility of the organs, and consequently produces a morbid alteration of their functions. Hence that dulness, that inactivity which we observe in individuals, who are, as it were, saturated with scrofula. Now if we take this view of the subject, and suppose all the other causes which are of a nature to

determine irritations, to be brought into operation on an individual so circumstanced, it is evident that the effects of the irritations which follow, cannot be the same as when the hæmorrhægia goes on in the normal way, and hence it is, that when the first condition, *foul air*, does not exist, the other causes will not determine the disease. The poorer classes in Palermo live on the most miserable food, and in filth and poverty; now, if these causes were what is generally ascribed to them, these people ought to suffer from scrofula. The contrary, however, is the case; and it is remarkable that they live in open air. What are we to infer but that notwithstanding their poverty, &c. &c., their breathing pure air exempts them from scrofula. In a word, I am satisfied that neither cold moist air, nor too nutritious food, nor poor food, nor inflammation, suffice to induce this disease without the action of foul air and the effect of the absence of the direct rays of the sun. Were this fact well established, of what importance would it not be to the manufacturing districts in our country, where this malady is so prevalent. From it would follow the necessity of keeping children and other labouring persons in well ventilated and well situated factories. I trust these remarks will direct the attention of observers who have an opportunity in these towns, to the consideration of this subject; and I have no doubt but that their researches will lead to such preventive means as shall, if not entirely suppress, at least considerably diminish, the frequency of this dreadful disease.

Scrofula affects more particularly the parts of the body in which the vital functions are most active. In conformity with a well known law of the economy, the liquids flow with more force, and in greater abundance, towards the head in children than in adults. Hence it follows that tubercles in the brain are more frequent in the former than the latter, because the brain in children is *functionally* the centre of a strong and continued action. However, this augmentation of vitality does not explain these morbid phenomena, for in old persons, although this

organ is the seat of extreme irritability, still tubercles are a very rare occurrence. Scrofula in general comes on in children at that period when a continued irritation is kept up about the jaws by the process of dentition. In adults we find the lungs affected with tubercles by reason of the activity of these organs at this period of life. In older persons by reason of the predominance of the abdominal viscera, tubercles of the liver prevail almost exclusively, and those of the uterus in women.

Some persons consider scrofula hereditary. This opinion cannot be admitted in the rigorous acception of the term; we inherit from our parents a disposition to contract disease with an organization like theirs, but not a germ of these diseases. The children of parents who present no trace of the disease may become affected with scrofula. Persons too old or too young, especially if their constitutions be impaired by excesses, give birth to children badly formed and disposed to many affections, more particularly to scrofula. As to this disease being contagious, there are too many opinions to the contrary on record, to render it necessary to insist longer on this point.

SYMPTOMS AND PROGRESS.

Preceded or not by redness of the eye-lids, and a swelling of the upper lip and nose, tumours appear, at first hard and moveable, afterwards fixed, indolent or scarcely painful. The heat of the skin is not more than natural, its colour not changed. These tumours occupy the sides of the neck under the jaws. They form masses which impede the motion of the jaw, and sometimes press on the trachea and vessels of the neck so as to be, though rarely, mistaken for aneurisms of these vessels. These tumours also appear in the groins and arm-pits. These may continue indolent for several months, and seldom disappear by spontaneous resolution. They more frequently become soft, after having considerably increased in size. In this last case, the skin becomes very hot, has a shining, bluish, or brownish red appearance, fluctuation becomes evident, ulceration follows,

and from the tumour, which in its progress presented all the characters of mild phlegmon, flows a thin serous pus *sui generis*. The ulcer has an irregular form, its borders elevated, hard and livid, its surface has a rosy colour, and is covered with exuberant granulations. The subcutaneous cellular tissue being in a great degree destroyed, a *decollement* of the skin takes place. This ulcer, when it does heal after a great lapse of time, is followed by an indelible and very deformed cicatrix. The tumours almost always contain large and numerous tubercles, and when they suppurate, yield, on pressure, a cheese-like substance, which leaves an immense and profound cavity after it. When the disease arrives at this period the viscera by way of sympathy contract similar affections; new diseases present themselves, with phenomena peculiar to their seat and the functions of these organs; and under their influence the patient becomes exhausted, falls into marasmus, and dies. The spongy extremities of the bones are often affected. Potts' disease may be considered of scrofulous origin, mesenteric and pulmonary tubercles belong also to it. When scrofula attacks the bones, the skin that covers them ulcerates, after having presented the ordinary characters of mild phlegmon. The borders of the ulcer are soft and inflamed, the pain exquisite, and the surrounding skin more or less affected. If you introduce a probe into the fistulous trajets, the skin is found deprived of its cellular tissue, and a sinuous passage leads to the bone, which also has lost its periosteum, and is, for the most part, considerably softened. These bones give a brown ichorous pus of a peculiar smell. The small articulations are most frequently attacked by this disease.

ITS PROGRESS.

Nothing can be more capricious than the progress of scrofula. These tumours diminish in one place and appear soon after in another. Scrofula may be complicated with syphilis, cancer, or scurvy. Scrofulous tumours, like schirrus and some venereal buboes, are at first indolent and hard, but they differ from

them. Schirrus ordinarily succeeds inflammation, and lancinating pains reveal its existence. When it ulcerates, the surrounding veins become large and of a dark colour, a foetid sanies flows from the ulcer, which is extremely painful. When scrofula is complicated with syphilis, there exist pain, lassitude, ulcers of the tonsils, pustules, gonorrhoea, chancres, crabs or phagaden, bleeding gums, carious teeth, and spots like ecchymoses indicate the existence of scurvy.

ITS TERMINATION.

This disease ordinarily terminates by resolution or suppuration. Death seldom occurs except from internal tubercles. The termination by gangrene seldom or never happens. Puberty often brings on a cure; there are, however, some exceptions; nevertheless, it is not less true that it has a happy influence on the progress of this affection. The same may be said of the effect of the spring season. At all events, the disease is very serious, as well on account of the slowness of cure and its deformed cicatrices, as the constancy with which the predisposition is transmitted from parents to their children.

TREATMENT.

In order to effect a cure, it is necessary to attend scrupulously and with perseverance to the precepts of hygiene. This is so true, that, if neglected, there is no success to be expected from any treatment, whereas if attended to, a cure is often obtained, no matter what anti-scrofulous remedies have been used. This fact is explained by the following, viz. that scrofula is almost always a consequence of the long continued non-observance of the laws of hygiene. The hygienic ought therefore precede the pharmaceutical treatment, not only because it often effects a cure itself, but also, as in the cases in which it does not entirely succeed, it advances the cure, whereas when neglected, all cure is impossible.

In the children's hospital the food given the patients consists of wine, bread, soup, and vegetables of good quality; and

every day, at least at one meal, meat, with all due regard, at the same time, to the age, appetite, and state of the digestive organs. The greatest cleanliness is observed. Baths and dry linen are abundantly supplied. Exercise in open air is eminently useful in scrofula. It affords the double advantage of quickening the circulation and improving the state of the functions, more particularly digestion. Were I not in possession of facts in favour of exercise, I would have enough to induce me to recommend it, in the authorities of Banne, who considers rest as fatal in this affection, and advises almost perpetual motion; and David, who assures having cured many patients affected with tumours in the neck, head, and fingers, by exercise alone. The analysis of many observations has convinced me, that scrofula will be inevitably aggravated, notwithstanding the punctuality with which the means just mentioned be executed, if the patient's dwelling be not exposed to the direct rays of the sun, and that it occupies a sombre, low, and narrow situation, where the atmosphere cannot be easily renewed. It is not necessary that the situation and dwelling be both unwholesome to bring on scrofula, one or the other being so is quite enough to determine this affection. It is impossible not to insist forcibly on these last points, when aware that what favours the development of the disease, is necessarily opposed to the means of cure. I have remarked that when the children were obliged, in consequence of rain, to remain all day in their wards, ophthalmia made its appearance, and cases that already existed became evidently worse with this change in the weather. Having thus spoken of the hygienic treatment, I will now advert to the medicinal agents. The long continued use of bitters, mercurials, anti-scorbutics, sulphureous baths, &c. &c., having produced no beneficial results, we therefore determined on making a choice from amongst the medicines so much spoken of in the cure of this disease. The first of these is iodine, a substance recently employed and to which the most wonderful cures have been ascribed. The second the ammoniacal hydrochlorate of copper, known by

the name of *liqueur de Koechlin*. Animal charcoal has also been tried in some glandular swellings, as likewise sulphuret of mercury combined with hemlock and magnesia, subcarbonate of potass, in fine, the muriate of baryta. The greater part of these medicines, from their uncertain effects, if not totally abandoned, are very little employed. My observations in this paper have for object, the effects of iodine, as being the medicine most frequently used. The extraordinary cures effected by it at the hospital of St. Louis, induced us to try its action on several children at a time.

The use of iodine requires the greatest prudence. It has been administered internally both in a simple form and combined with iron, mercury, and potass, in the form of pill or in solution. Externally combined with lead under the form of ointment, or dissolved in water in its simple state, either in lotion, injection, or bath. All these means have had their respective advantages. They were consequently used on various occasions. Iodine taken internally constitutes the basis of the treatment where the stomach is not affected. The results of cutaneous absorption are too doubtful to admit of frictions as the principal form of administering iodine. Children take pills with much difficulty. Pure iodine may be given in the form of tincture diluted with water. Two reasons, however, prevent us from using the tincture at this hospital. In the first place the fear of mistakes in dropping, mistakes the more easily made when there are many children under treatment; and secondly, our apprehensions lest the water of Arcueil (which is used exclusively in this establishment, and which contains a large quantity of calcareous salts,) may alter the nature of this medicine. The same objection holds good with regard to the æthereal tincture of iodine, the simple or ioduretted solution of hydriodate of potass, and the alcoholic æthereal tincture of ioduret of mercury. We use the solution of iodine in distilled water called by Lugol, "*Eau minerale iodée*." Each pint of water contains two grains of iodine and four grains of hydriodate of potass, the

latter being added to render the former more soluble. We have not deemed it prudent to adopt the use of the solutions of different degrees of strength which Lugol employs; because in the treatment of children, particularly when they are numerous, these different proportions offer many inconveniences without any real advantage. It is much more simple to administer a solution that contains a fixed quantity of iodine, which may be prescribed at will in more or less large doses, than to have for each patient solutions that contain a variable quantity of iodine.

The dose varies with the age, the state of the digestive canal, its influence on the disease, always beginning with three ounces of the solution, which may be gradually augmented to twelve ounces in the day. This is the strongest dose we have given, viz. \mathfrak{zvi} . in the morning and \mathfrak{zvi} . in the evening. Each ounce contains the $\frac{1}{8}$ of a grain of iodine and $\frac{1}{4}$ of a grain of hydriodate of potass. In following this method we know the exact quantity prescribed. After the use of this proportion for some time, we thought to double the quantity of iodine for each pint, but were soon obliged to abandon it in consequence of the disagreeable taste of the medicine, and a sensation of heat which it occasioned in the throat. The solution may be sweetened immediately before use with sirup of gum. If mixed with this sirup long before hand, the iodine becomes decomposed, and the solution loses both its colour and taste. In order to preserve it long fit for use, it should be kept in bottles well corked and opened as seldom as possible. The smallest dose to begin with is \mathfrak{zi} . morning and evening, which may be increased to \mathfrak{zvi} . each time as before mentioned. This dose is even stronger than that Lugol recommends for adults. When no accidents occur to contra-indicate its use, we continue this solution during four or five weeks, then we stop it, and give a purge of sulphate of soda or magnesia. The purgative is repeated two or three times before we return to the use of the solution. The suspension continues generally fifteen or twenty days. We then resume, and continue its use a month, then stop it again to give

the purgative as before said. It sometimes happens that acute accidental diseases oblige us to discontinue the treatment for some time. Diarrhoea and emaciation, which have been so much dreaded, seldom occur; when they do, they always cease on stopping the medicine. I have seen but one case in which the patient became thin; all the other children, on the contrary, have become very fat. The appetite has increased, and their soft flesh become hard and coloured. I have seen a case in which the use of iodine was followed by cardialgia, and this affection ceased on using $\mathfrak{z}\text{i}$. of bark once a day, without discontinuing the iodine; and after a short time we were able to give the medicine alone. It sometimes happens, though seldom, that the use of iodine ulcerates the mouth and affects the breath as in mercurial salivations. In some females symptoms of cerebral congestion appears, such as epistaxis, which yielded to rational treatment. It is remarkable that these symptoms manifested themselves at the period of puberty, and consequently may be ascribed more rationally to a deviation of the menstrual flux, than to any other cause. Messrs. Gardner and Coindet attribute to a kind of iodine saturation, accidents which have been remarked, in some patients, such as fevers, palpitation, dry cough, tremblings, loss of strength, and swelling of the legs. At the Children's Hospital we have not remarked such cases. This may be owing to the precaution we use of purging the children from time to time. Moreover, these symptoms are extremely frequent in the third stage of phthisis in patients who never used iodine. The ioduret of iron may be given with good effect in this affection, more particularly in cases where the patients are very pale, and their flesh soft and flabby. This medicine may be given in larger doses than the hydriodate of potass. We begin by half a grain in $\mathfrak{z}\text{i}$. of water morning and evening, and increase it half a grain every four days. It may be increased to ten grains a day; in higher doses it induces vomiting. The solution of ioduret of iron produces diarrhoea much more frequently than the iodine solution. Iodine frictions are

very useful auxiliaries to its action internally. We rub the tumours with an ointment composed either of ioduret of lead, ioduret of mercury, or ioduret of potassium. It sometimes happens, (as with other medicines in all diseases,) that the ointment first used brings the tumour to a certain degree of diminution, and then it remains stationary. In such cases it is useful to substitute one of the others, and by this means the absorbent system is kept in a state of salutary excitement. The ointment of ioduret of mercury is composed of ℥i. axunge, and ʒss. of ioduret of mercury, that of lead of ʒi. to ʒi. of axunge, and that of ointment of hydriodate of potass with iodine, of ʒi. hydriodate and twelve grains of pure iodine to each ounce of axunge. The ointments of ioduret of mercury and potass determine a pricking pain that continues fifteen minutes, that of lead does not produce this sensation although applied to ulcers, spread on linen or charpie. A child ought to be rubbed but once a day, and an adult twice, in consequence of the skin being less irritable, and absorption less active in the latter; each friction during five or six minutes; and the quantity of the ointment varies with the size of the tumour, the minimum being that of a small pear or nut. We have in no case relied on frictions as the sole means of cure. They have always been combined with iodine internally and in baths. It is a matter of perfect indifference to which of the ointments you give a preference. For injecting fistulous trajets we use a solution of twelve grains iodine and twenty-four hydriodate of potass in one pint of water.

Caustic iodine, (made by dissolving equal parts of iodine and ioduret of potassium in a quantity of water equal to the other two substances united,) when applied to the skin, determines a good deal of smarting, and when it is put on ulcerated surfaces three times a week, it acts as a desiccative. To a bath that contains 300 litres of water, we put ʒiiss. of iodine and ʒv. of ioduret of potassium, dissolved previously in ℥ii. of rain water, and to one that contains 250 litres, ʒii. of iodine, and ʒiv.

of ioduret of potassium. Many children may be put into one of these baths together; it matters not what form of the disease they present. Some patients, during and after the bath, feel pain on the ulcerated surfaces, and in some instances a painful redness of the skin comes on. This redness may be owing to the high temperature of the water, a circumstance which increases the action of iodine. These trifling accidents seldom occur, and are very trifling in their effects. The bath almost always produces an abundant diaphoresis, followed by sleep, and the ulcers present for several hours a very strong tendency to cicatrize, and assume an appearance similar to that brought on by the use of chloride of lime as a topic when applied to burns; nevertheless, the purulent discharge, although less abundant, re-appears after a short time. The smell of iodine has a salutary influence on the air of the wards. It is remarkable that sulphureous baths have never produced a similar effect. We leave the patients in the bath as long as transpiration is not extreme, and their faces not much injected. When the tincture is used, six ounces of it are to be added to 300 litres of water; this quantity is equivalent to 3m. of iodine. It is first mixed with 1lbiss. of water, and then added to the bath. No precipitate takes place, and it produces the same effect as the other preparations.

Sixty-seven female children have been treated with iodine for a period sufficiently long to enable us to determine its effects with precision. Their age varied from four to fifteen years, a little more than half were over ten years old. In all of them the disease was of long standing; one was in the hospital since 1822, two since 1826, six since 1828, ten since 1829, twenty-six since 1830, and thirty-two were admitted since 1831, either before or after the 1st April, at which period the treatment commenced. In all these children the disease presented itself under different forms. Fifteen of the sixty-seven who were treated by iodine were discharged cured of the apparent symptoms of the disease; fourteen experienced considerable amend-

ment, which announced a speedily approaching cure. In thirteen others, although the improvement was less evident, one could judge that the cure would take place at some not far distant period; five received but little benefit from it, and, in fine, twenty did not receive the slightest advantage from the use of this medicine. Among the children cured was one admitted in 1826, two in 1829, seven in 1830, and five in 1831. Among those who experienced considerable benefit, was one admitted in 1826; two in 1828, three in 1829, five in 1830, and three in 1831. Of the twenty who were not at all relieved, two were in hospital since 1828, four since 1829, seven since 1830, and the remaining seven since 1831. All these children, with a few solitary exceptions, were much more affected at the period they commenced the use of iodine than at the time of their admission. The disease had increased with their sojourn of many months, and even years, in the wards.

I have entered into all this detail to satisfy those who are of opinion that this affection has a determinate existence, and that after a certain time we obtain a cure, no matter what medicines we use. Now, in analyzing those facts, it becomes evident that if two patients admitted in 1826 were cured, another admitted in 1822 scarcely experienced any benefit. In comparing the numbers, we also find that the old date of the affection ought to be considered as having little influence in bringing about the marked improvement or speedy cures that were obtained.

Seventeen of the sixty-seven submitted to this treatment had glandular swellings. In only four of these, the tumours entirely disappeared. Iodine was used in bath, friction, and solution, given internally. In one case, the treatment reduced a very large tumour to three small ones in a very short time; these small ones remained stationary. How are we to explain this obstinacy, this resistance to the action of a medicine which dissipated, and that so rapidly, the nineteen-twentieths of this swelling? The following is probably the reason: These tumours constitute the nucleus of the affection; they are at first

isolated and distinct, and by their presence inflame the cellular tissue which surrounds them. This inflammation is followed by the induration and increase of volume of this tissue, and hence it is that after a time they all form one compact mass. Now, iodine, in whatever form it is used, acts specially on the absorbent system, and when rubbed on these tumours, promotes the resolution at first of the indurated cellular structure, and subsequently of the enlarged glands, if these glands be but simply enlarged. Thus in some such cases the tumours entirely disappear; but, if, instead of being simply indurated, these glands contain tubercular matter in their substance, as this matter does not appear susceptible of resorption, the absorbent vessels, whose action has been increased by the medicine just mentioned, produce no action on them. These tubercles remain after the simple glandular and cellular indurations have disappeared, until an irritation ensues, (induced either by their presence in a structure abundantly provided with nerves and blood-vessels, or probably by the irritating applications which have been used with a view to bring on resolution), and this irritation is followed by phlegmonous inflammation, abscess, ulceration, and finally, by the elimination of the tubercular matter, which was a component part of these tumours.

Three children were cured of small tumours on the neck. In thirteen cases the iodine completely failed, inasmuch as that the tumours either did not diminish, or that inflammation came on, followed by abscess. Many tumours terminated in this way, whether it was that the iodine produced this effect on them, or could not prevent this termination. Of sixteen children affected with tumours which terminated in abscess, with whom we used iodine, only eight were cured. In the treatment of the ulcers subsequent to these abscesses, to the other means of cure we added injections of iodine solutions into the purulent sinuses. The ulcerated surfaces were covered with pledgets of lint with ointment of ioduret of lead, or imbibed with a solution of iodine. In eight children there was no

amendment produced after many months' treatment; in eight others a complete cure of the swelling was obtained. In some cases of ulceration, notwithstanding the absence of all swelling, the most methodical dressings, injections, and well applied pressure, the skin will not adhere to the subjacent tissue. In order to obtain a perfect cure in such cases, it is necessary to lay bare all the ulcerated surface by destroying the loose skin until you arrive at the neighbouring healthy skin, either with a caustic application or the cutting instrument. The sore then heals rapidly, and in no case has the cicatrix presented that thickness, irregularity, and deformity, so frequent when the ulcer is left to nature, and which are the "unequivocal" marks of a disease which patients would often wish to conceal.

In scrofulous ulcers the skin is so bare and thinned that it cicatrizes with the greatest difficulty, and although the bottom of the sinuses be long since deterged and disposed to unite with the skin, the union does not take place, the skin not being in the condition indispensable to such a change. After many useless efforts to unite the parietes of these sinuses, nature cicatrizes them separately.

Now, if we bear in mind the mechanism of the formation of cicatrices, the dragging the surrounding parts, experience from the flattening (*affaissement*) and contraction of the granulations, we will easily understand how the skin shrivels, doubles on itself, and at the same time presses forcibly against the cicatrix which covers the bottom of the sinus. In this case, there exist two cicatrices superimposed. When the cicatrization, instead of commencing at the bottom of the sinus, first takes place on the skin, the skin doubling on itself and contracting, as in the former case, presses firmly against the ulcerated surface, vegetations arise around it, surpass its level, and when they begin to heal, encase it, as it were, constituting these elevations or scrofulous cicatrices which the French call *bourbiers*.

The skin ought never be removed until the glandular swelling has disappeared, otherwise it will be necessary to repeat

the operation. Even when the "engorgement" has ceased to exist, before having recourse to either caustic or bistoury in order to remove the skin, stimulating injections and pressure may be tried, and the probability of success from these means considerably increases if the skin has preserved a certain degree of thickness, and that its colour does not differ from that of the surrounding parts. Although such attempts are sometimes fruitless, they are not to be overlooked, especially when the ulcer occupies a part of the body which is constantly exposed to view, and where consequently it is necessary to obtain a cicatrix that will present the least possible extent. When no good results follow these remedies after ten days' use, it will then be necessary to have recourse to the caustic or cutting instrument. The destruction of the skin in this way offers the double advantage of a rapid cure and an undeformed cicatrix.

When left to nature, these ulcers take many months, and even years, to heal; and even then the cicatrix will be deformed, large, and characteristic. On the contrary, when art interferes, the cure is obtained in fifteen days or a month at farthest. The cicatrix is smooth, and differs in no manner from that which follows a simple wound with loss of substance. When it is deemed necessary to destroy the skin, the operation ought to extend to the limits of the *décollement*. As I have before said, we use indifferently the caustic or bistoury. The following is the caustic preparation which I have frequently applied with success: Powdered quick lime, six drachms; caustic potass, five drachms: powder the potass in an iron mortar, adding gradually the lime. This gives a fine powder, which is to be kept in a bottle hermetically closed. When about to use this caustic, make a paste of the powder with spirit of wine, and apply it in a layer of about two lines thickness on the part which it is intended to cauterize. The eschar will be of the exact extent and thickness of the layer applied, and in six minutes the skin is cauterized to the subjacent cellular tissue.

The paste is then removed, unless it be intended to produce a more profound eschar.

These considerations on the cicatrization and local treatment of scrofulous ulcers are quite independent of the general treatment. They are applicable to these ulcers, no matter what other treatment is used.

It appears to me, from this *exposé* of the effects of iodine, that we are not to consider the results obtained as merely attributable to a fortuitous coincidence of the administration of this remedy with the cure of the disease, but rather the action of one on the other.

In three cases of periostosis a rapid cure has attested the efficacy of iodine. The diseases of the bone yield less speedily than those of the soft parts, owing to the slowness with which the vital functions go on in them. Lesions of these parts frequently occur in scrofula under the form of swellings, a regular *hypertrophy* of this tissue. Caries is also very frequent. Necrosis is not rare in scrofula, although some authors hold a contrary opinion. Swellings of the articular extremities are also frequent, swellings which render the motions of these articulations extremely difficult, thereby constituting *false anchylosis*. These swellings are accompanied by inflammation of the surrounding cellular tissue and abscesses, followed by numerous fistulous trajets. Masses of tubercles are often found round the joints, and these tubercles give rise to abscesses and ulcers.

In thirty cases of caries I have seen but four cures; twelve experienced considerable improvement. This proves that iodine is not less effectual in the treatment of this form of the disease, (taking into account the organization of bones), having produced a beneficial effect on nearly half the cases subjected to its use. Necrosis of the bones of the tarsus and metatarsus, of the phalanges of the fingers and toes, is not rare in scrofula, and iodine in such cases stops the caries and diminishes considerably the suppuration. In order to obtain this end,

abundant injections and the use of iodine by the mouth are necessary.

When the new bone is large, after the sequestrum has been removed, its size may be diminished by moderate pressure by means of strips of adhesive plaster tightly rolled round it.

Esthiomena (lupus) is one of the forms of scrofula which does not yield to iodine in any form. I know of two cures obtained by the exhibition of arseniate of soda internally, and as an external application, an ointment composed of orpiment, quick lime, and caustic potass. As a caustic, this ointment may be applied to a surface as large as a shilling without danger. I have given three-fourths of a grain of arseniate of soda to children, beginning with the one-sixteenth of a grain, as a dose, but not more, without inconvenience. In scrofulous ophthalmia (as in many other chronic diseases) the last remedy applied often appears to be that which has produced most benefit, whereas it frequently has no other merit than that of being administered when nature was about to effect a cure. Thus, ophthalmia often comes on without any assignable cause, and disappears in like manner. The transition from dry to cold and moist weather renders this ophthalmia most frequent. This I have remarked at the children's hospital. When, on the contrary, we have a continuance of dry weather, the disease disappears under the influence of medicines that were hitherto utterly ineffectual. When this ophthalmia assumes an inflammatory character, local and general bleedings are useful. The intensity of the disease is diminished, but the cure is seldom effected by these means alone. It persists in a chronic form for an indefinite period in spite of the most rational treatment. The same is observed in originally chronic scrofulous ophthalmia. Emetics, purgatives often repeated, blisters, setons, collyria of all kinds, may completely fail, and the disease disappear in a few days spontaneously, a little after all curative means had been suspended. When this disease attacks the ocular conjunctiva, Sydenham's laudanum dropped in alternately, with some few grains of calo-

mel blown into the eyes, the one in the morning and the other in the evening, have been more useful than any other remedy. When the borders of the eye-lids are red and swollen, and that the glands of Meibomius secrete a purulent liquid which dries on the lids, the ointment of red oxide of mercury diluted is an excellent application. It is composed of 3ss. of red oxide, and ℥i. of unguentum rosat. a little to be rubbed to the lids at night. Iodine is of little or no benefit in the treatment of these ophthalmias. Staphyloma and considerable deformity of the eyes, ulcers of the cornea, are frequent consequences of these ophthalmias; specks on the cornea, and even ulcerations, when superficial, of this membrane, have been cured by the use of calomel and laudanum applications. Purulent discharges from the ears, nose, and vulva are frequent in scrofula, and in such cases iodine injections may be used frequently in the day with the best effect.

In fine, it may be inferred from all these facts, that without ascribing to iodine, in the treatment of scrofula, specific properties of a decided nature, such as those exerted by bark in ague, we must nevertheless admit that no other remedy hitherto used in scrofula is so often or so generally successful as iodine.

ART. XXI.—*Report of the Obstetric Practice of the Wellesley Female Institution during the Year 1833.* By HENRY MAUNSELL, M. D., Accoucheur to the Institution, Assistant Physician to the Magdalen Asylum, and to the Institution for Diseases of Children, and Lecturer on Midwifery, &c. at the Medical School, Park-street, Dublin.

IN the present report I shall pursue the same plan as was adopted in that for the year 1832, (published in the Edinburgh Med. and Surg. Journal, No. 117), stating in the first place the statistical facts which we were able to observe, and, secondly,

giving brief abstracts of any thing remarkable in practice which came within our notice.

The total number of cases attended from the 1st January, 1833, to the 1st January, 1834, amounted to 428. Of these, twelve were abortions, leaving a balance of 416 labours. From these were produced 421 children, there having been five cases of twins, or one in 83½. The proportion of the sexes among the children was 256 males and 165 females; a very remarkable difference, and the more so, that in the births occurring in the institution during the year 1832 (and noticed in the last report) the males were to the females as 211 to 220. The number of dead born children amounted to thirty-five, or one in twelve, of which twenty-four were male, and eleven female, many being, as in the former year, premature.

The ages of the 428 women attended were as follows:—

Under 20 years,	24 women.
Between 20 and 25,	124 ———
———— 25 and 30,	164 ———
———— 30 and 35,	58 ———
———— 35 and 45,	57 ———
Of 47 years,	1 ———

The duration of labour in the 416 labour cases was according to the following table:—

Of 6 hours and under,	147 cases.
Between 6 and 12 hours,	154 ———
———— 12 and 18 ———,	52 ———
———— 18 and 24 ———,	35 ———
———— 24 and 48 ———,	21 ———
Of 56 hours,	3 ———
———— 64 ———,	3 ———
———— 72 ———,	1 ———

In the 411 cases of single births, the presentations were—

Natural . . . 399, including 5 of the face and 1 with the face towards the pubis.

Of the breech	6
— feet	6
— knee	1

Total of the lower extremities 13, or 1 in 31 $\frac{8}{11}$.

Of the upper	0
— placenta	3, or 1 in 137.
— funis	1, or 1 in 411.

In the five twin cases the presentations were natural in two cases. First child, of the lower extremities; and second, natural in one case.

Both of the lower extremities in two cases.

The sexes in these were—

In 3 cases both females.

In 1 case — males.

In 1 — one of each.

The perforator was employed four times, but as one of the operations was for the liberation of the head of a dead child in a breech presentation, I shall take three as the number necessarily performed, which gives an average of 1 in 137 cases.

The forceps was attempted to be used in one case (a face presentation) but failed, and the perforator had ultimately to be resorted to.

In three cases, or 1 in 137, there was severe hemorrhage after the birth of the child; all terminated favourably.

In two cases, or 1 in 205 $\frac{1}{2}$, the placenta was retained, *without hemorrhage*, and required manual extraction.

There were three cases of convulsions; 1 in 137.

Of the whole number of 428 women entered upon the books, 44, or nearly 1 in 10, are marked as having had previous abortions.

Crotchet Cases.—In three of these cases the women recovered perfectly without a bad symptom. In one (as has been already noticed) perforation was employed merely for the pur-

pose of facilitating the passage of the head of a dead child, which had presented with the breech. In the second case (a first labour) the woman was sixty-four hours ill, all uterine action had ceased, and from the condition of the scalp and overlapping of the bones of the head, it was manifest that the child had been for some time dead. The operation was easily accomplished, and the woman recovered very well.

The third case was one of face presentation, the child being very large. The labour was not interfered with during thirty-four hours, at the termination of which period the pains became very weak and quite ineffectual; the soft parts were beginning to suffer, the right labium being swoln, and the epidermis, on its inner side, raised into a blister. The upper part of the neck, chin, and mouth of the child presented, and together with the tongue, which protruded between the lips, were livid and exceedingly swoln. No advance had been made for several hours, but during the last effective pains there appeared to be a tendency to force down the neck of the child anteriorly, and to press the forehead backwards, out of reach of the finger, into the hollow of the sacrum. Under these circumstances, as the woman had had four children, and there appeared to be no remarkable want of room, it was determined to attempt delivery by the forceps. Only one blade, however, could be fairly introduced, which was passed between the head and sacrum, and used as a vectis for more than an hour without the slightest effect. As the woman was now getting considerably weaker, and the pains had entirely ceased, she was delivered by the crotchet, the perforator being introduced through the orbit. Very considerable force was required, even after the brain had been completely evacuated. The woman was threatened with peritonitis, but ultimately recovered. The fourth crotchet case terminated unfavourably. The patient was 25 years old, and in labour of her first child. When she was visited from the institution she had been under the care of a midwife for several hours, the pains were found to be very strong and violent, and

the head firmly wedged into the brim of the pelvis, with a portion of the anterior lip of the os uteri caught between it and the symphysis pubis. The woman was flushed and restless, pulse 120, strong and full, and her bowels had been confined for several days. There was no abdominal tenderness, and she had made water freely; but her countenance expressed a peculiar anxiety, which, coupled with the obstinate costiveness, alarmed me with respect to her from the first moment I saw her. A bolus of calomel and jalap was administered, followed in a short time by an enema, and ℥xviii. of blood were taken from her arm. This was at 11 o'clock on Sunday evening (Dec. 9). Some effect was produced by the purgative, and the extreme violence of the pains was a little diminished. At 8, A. M. on Monday, the presentation was exactly in the same state, the pains continuing regular and strong, although not so violent as on the preceding night; the pulse 120 and strong; tongue soft and clean; countenance still anxious. A catheter was introduced, and some brownish urine drawn off. At 1, P. M., matters were precisely in the same state; the foetal heart could be plainly distinguished with the stethoscope. At 3, P. M., there was no change in the symptoms or position of the head, but as the pains had become weaker and the pulse 140, and seventy-two hours had elapsed from the beginning of labour, it was determined to deliver her. There was considerable deficiency of room, but the head, being well emptied, was extracted without any violence to the soft parts. The placenta was expelled naturally in a few minutes, and shortly afterwards the woman was seized with a rigor which lasted twenty minutes. Ten grains of Dover's powder, and two of calomel, were given at bed time. On the next day (Tuesday) there was some slight tenderness over the uterus; the pulse were 130, and the countenance anxious, but there had been no return of rigor, and the tongue was clean and moist: a grain of calomel and a third of a grain of opium was given every fourth hour. In the evening the abdomen was stuped with turpentine, ℥xii. of blood ab-

tracted, and the calomel and Dover's powder given as on the preceding evening. From this period forward the abdomen remained soft without the slightest tenderness, except the uterus itself was very firmly grasped; there was no return of rigor, and the tongue continued perfectly clean and natural. The pulse, however, never fell below 130, and the countenance still retained the peculiar anxious look which had attracted our notice from the beginning. On the third day after delivery, a feculent diarrhoea set in, for which enemata of starch and laudanum were administered without any effect. It was not determined to endeavour to salivate her quickly, and four grains of calomel and one of opium were given every four hours; the first effect of which was to check the diarrhoea and procure sleep. In this way upwards of a drachm of calomel was given without in the slightest degree affecting the mouth. The symptoms remained precisely as last stated, the woman gradually getting weaker. On the tenth day the feculent diarrhoea returned, and on the twelfth, without any aggravation of symptoms, her stomach became sick, she vomited a little, and immediately expired. There was no lochial or other discharge from the vagina after the second day, and the urine was freely evacuated. Was this a case of inflammation of the uterine substance? Unfortunately no *post mortem* examination could be obtained.

The presentations of the lower extremities all went on favourably as far as regarded the mother. They were nearly twice as numerous as in the year 1832, while, on the other hand, there were this year no presentations of the upper extremities.

In the cases of *placenta presentation* the mothers' lives were all saved. In two, turning was performed. In all three, the children were dead. In one of the cases in which turning was resorted to, the plug was employed with marked advantage. The woman was in the beginning of the eighth month of pregnancy, without the slightest appearance of labour, and with an undilated os uteri. She had been losing blood occasionally

for several weeks, when, on the 3rd April, at 3, p. m., a larger gush than usual having occurred, a plug saturated with vinegar was introduced fully into the vagina. This restrained the hemorrhage until 7 o'clock, when pains came on, and the plug was expelled, followed by a violent discharge, upon which the hand was immediately introduced, and delivery effected. In the case in which turning was not performed, the woman, at 40, at the full period of pregnancy, was seized with hemorrhage on the 6th of May, which continued occasionally until the 8th, when she sent to the institution. The pupil who visited her found her with a bleached countenance, very feeble pulse, cold extremities, and a smart draining of blood from the vagina. He immediately introduced a plug, and sent for me. On examination, the placenta was found within half a finger's length of the external orifice, completely blocking up the vagina. It appeared to have passed completely through the os uteri, which could not be felt. There was now no bleeding whatsoever, although the woman had regular pains. Upon prosecuting the examination at the anterior part of the vagina, for the purpose of ascertaining what was beyond the placenta, the membranes were ruptured, and the head felt. The pains continued regularly, though weakly, until the placenta could be seen protruding through the vulva. By degrees the head was forced under the arch of the pubis, and in proportion as it gradually advanced, the placenta was pressed backwards and upwards into the hollow of the sacrum, until the head filling up the whole vulva the other could no longer be felt. A dead child was naturally expelled, and the placenta found loose in the vagina. It was flattened as if it had been subjected to considerable pressure.

In two of the cases of *hemorrhage* the placenta was extracted. In the third the hemorrhage was caused by the patient standing up in an hour and a half after a complete but very sudden delivery. It produced very alarming fainting fits, but was effectually restrained by external pressure.

Convulsions.—In one case (seen by Dr. Churchill) the con-

visions came on at the full period of pregnancy. The woman was at first sensible between the paroxysms, and complained of headach, with a quick strong pulse. She was bled twice to the amount of ℥xxvi. , her head shaved, and cold applied to it, and the membranes were ruptured. In a few hours she was delivered of a dead child. The fits, however, still continued every half hour until the next day, when a blister was applied to the nape of the neck, and ℥xx. of blood taken from the temporal artery. Purgatives were also given, and subsequently enemata, with tincture of opium and assafoetida. She perfectly recovered, but without the slightest recollection of any circumstance connected with her illness.

In the second case the woman had a natural (first) labour of twelve hours' duration. During her pregnancy she had suffered very much from edema of the lower extremities, which prevented her from taking exercise. In seven hours after her delivery of a healthy child, she was seized with violent convulsions, which recurred very frequently, leaving her in the intervals perfectly insensible, with a quick full pulse, (about 150), and pale, leuco-phlegmatic face. She was bled to ℥xvi. , a bolus of calomel and jalap laid upon her tongue, a turpentine enema administered, and her head shaved and kept cool. The fits continued, but at longer intervals, during the day and night. In the evening a blister was applied to the head, the enema repeated, and the abdomen stuped with turpentine. The bowels were opened, after which she had but one fit. On the following morning she lay in a drowsy state, but could be roused to answer questions, and eagerly took drink. She from this time gradually improved and recovered well, but without retaining the slightest recollection of her labour, or of any event which had occurred during three or four days previously, although she was then apparently free from all affection of the head.

The third case was a fatal one. The woman was about 40, strong looking, with a thick short neck, and leuco-phlegmatic face. She was in the sixth month of her seventh pregnancy,

and had been prematurely delivered (at the same period of gestation) of all her children except the first. We heard that she had had convulsions in her last labour but one. At 3, A.M., December 10, she was seized with pain and tenderness in the epigastrium, restlessness, and retching. Her pulse were quick and hard, tongue white, but no headach. Dr. G. Thorpe, who was then a pupil of the institution, saw her immediately, and took from a large orifice $\frac{3}{4}$ xv. of blood. Warm stupes were applied to the abdomen, which relieved her for a short time, but at half past six she was seized with fits, which recurred every half hour without any interval of sensibility. At 10 o'clock she was again bled to $\frac{3}{4}$ x., a turpentine enema administered; her head shaved and kept cool, and a bolus of calomel and jalap placed upon her tongue. At 11, successive doses of a solution of tartarized antimony were poured into her mouth, which produced vomiting. At 3, P.M., the paroxysms had become more frequent, the pulse were very quick and irregular; the pupils insensible, one being dilated and the other contracted, the breathing stertorous; she was again bled to $\frac{3}{4}$ x. At 4 there was no amelioration; the membranes were ruptured, and a blister applied to her head. At 9, P.M., a female foetus was expelled during a convulsion. After this she had only three paroxysms, but she remained quite insensible, and had no evacuation from the bowels. At 3 on the following day (thirty-six hours after the first attack) she expired. No entreaties could prevail upon her friends to allow of a *post mortem* examination.

Imperforate Rectum.—An instance of this malformation was met with during the year. The child (a female) was apparently well formed, the anus being open and perfect. It had no evacuation after birth, notwithstanding the administration of a spoonful of castor oil. On the second day the abdomen began to swell, the child vomited meconium, and had two or three fits of convulsions. Some attempts at throwing up enemata having failed, I was sent for, and upon introducing a gum elastic

catheter, found that the rectum was completely closed at the distance of one inch from the anus, forming a perfect *cul de sac*. An operation was recommended, but from the unwillingness of the friends, was not performed until the fourth morning after birth. I then opened the anus with a dressing forceps, and passed up a probe in the presumed direction of the gut. When it had gone about an inch beyond the obstruction, a little air was observed to escape; it was then withdrawn, and a director introduced, along which a trocar was pushed; the canula was allowed to remain in the passage, and through it a large quantity of meconium immediately passed, and continued passing for several hours. The child lived two days, the gut remaining pervious, but ultimately died in a fit.

Only two deaths of mothers are recorded during the year, both of which have been already noticed.

Before concluding this report, I am anxious to make a few remarks which, no doubt, must be very imperfect, but which are put forward now as the subject is fresh in my mind, and an opportunity for noticing it may not again occur. I wish to offer to the profession a friendly caution against the *indiscriminate and inordinate* employment of ergot of rye, and in doing so, desire to be understood as in no way retracting the favourable opinion of its prudent use expressed in my former report. What I mean to say at present, is, that extraordinary doses of this drug occasionally produce dangerous effects upon the nervous system, and probably other mischief, and at the same time are less likely to act upon the uterus than smaller quantities.

Mary Redmond, in the fourth month of pregnancy, was seized after a fright and severe exercise with hemorrhage from the vagina, which continued at intervals for several days. On the 10th March, 1834, at 6, P. M., Dr. Churchill saw her and gave forty-five drops of tincture of opium; at 9, P. M., she was so weak that a plug was introduced. At 7 o'clock on the morning of the 11th, the plug was expelled with considerable he-

hemorrhage, and Dr. Churchill ordered a grain of opium and two table spoonsful of acid purgative mixture every second hour. At 8, P. M. of the 11th, hemorrhage set in again, and Dr. Churchill gave 3ss. of the ergot, and repeated it in half an hour. On the 12th, she had violent headach, and in the course of the day became delirious, and could with difficulty be kept in bed. These symptoms yielded to purgatives, shaving and blistering the head, &c. In a few days afterwards the hemorrhage returned, ergot was again given, and again followed by delirium, although in a slighter degree.

Mrs. Forrest, two months pregnant, was attacked with hemorrhage from the vagina on Thursday, the 21st April, for which she was kept quiet, cold applied to the vulva, and the acid purgative mixture given. On Friday the discharge returned in an alarming degree, and the infusion and substance of 3ss. of ergot was given to her by Dr. Churchill in two doses, with an interval of fifteen minutes. After taking it she fell asleep, and when she awoke the discharge returned; another scruple of ergot was then given, in an hour after which she vomited. On the following day she was in a state of half stupor, with very violent headach and weak, depressed pulse. Her head being shaved, &c., and purgatives administered, these symptoms gradually gave way.

The occurrence of these two cases attracted my attention to the subject, and upon inquiring among my friends I found that they did not stand alone. Part of the information which I received, I shall now briefly recite.

A case was related to me by Dr. Johnson, in which labour of a first child was rendered tedious by the want of uterine action; as the woman had a well formed pelvis, the head had passed the os uteri, the soft parts were cool and well dilated, and there was no fever, it was thought advisable to give the ergot. Through a mistake of the person in immediate attendance the infusion and substance of two drachms of powdered ergot was given in two doses. In six hours Dr. J. was again

called, and found the patient in a state of incomplete coma, with livid face, and muttering delirium. His first impression was that she had had a convulsion, but finding her pulse reduced to a thread, and only seventy in a minute, he was induced (by his having known similar depression of pulse to follow the administration of ergot) to make further inquiry, and then learned the mistake respecting the double dose. There was no uterine action whatever. She was delivered by the forceps, and remained thirty hours delirious and in a state of partial stupor. She was afterwards threatened with peritonitis, and had a bad recovery. In another case in which ergot was used largely, Dr. Johnson saw complete gangrene of the external parts and death occur six days after labour. In a third he witnessed extensive sloughing of the vagina, without any other probable cause. In neither of these two cases were any instruments used. Attributing such results as these latter to the abuse of ergot may appear to be a mistake of the *post* for the *propter hoc*, but as we know that gangrene is a consequence of its employment as an article of food,* they should at least make us cautious. The following extract of a note which I had the pleasure of receiving from Dr. S. Cusack, contains some interesting statements. He says:—"Not having notes of the cases in which I used the ergot of rye, I shall briefly state from recollection my experience of it. I was formerly in the habit of giving the infusion and substance of half a drachm, and repeating the same dose at intervals of ten minutes. In one case, supposed at the time to be favourable, the quantity stated above was given three times; the pulse came down from 120 to 90, considerable stupor and epistaxis supervened, but no uterine action whatsoever. The patient was ultimately delivered with the crotchet. In a second case the pulse came down even below the natural standard, a similar tendency to coma existed, and as no uterine action was excited, the child (an acepha-

* Vide Thomson on Inflammation.

lous one) was delivered by the forceps. I have since used the ergot in ten grain doses, and I think with more effect.

"Yours, &c."

"S. CUSACK."

"May 1."

Upon looking over a short essay upon the use of internal medicines in parturition by Professor Joerg of Leipzig,* which was sent to me lately by my friend Dr. E. Joerg, I find the views referred to above corroborated by the results of experiments with ergot performed upon the stomachs of the Professor himself and several of his pupils as well as upon animals. When taken in large doses it was always found to produce nausea, and vomiting, pains in the abdomen and diarrhoea, weight and pain in the head and vertigo, with general *mal aise*, and depression. Upon animals it produced similar effects, and in the smaller animals, (as doves and cocks,) a comparatively small dose caused death, which was preceded by coldness and lividity of the extremities, and often accompanied by convulsions. I cannot, however, agree with the learned Professor in condemning its use altogether, nor am I convinced of its inefficacy by the pun in which he affirms that he can see no relation between *mutter-korn*, (ergot,) and *gebär-mutter*, (the womb,) except the word *mutter*, which is common to both. I shall leave the foregoing facts to speak for themselves; they are not sufficient foundation for a theory, neither is it my wish to erect one upon them; they are merely put forward in the hope that they may act as a drag upon the destructive career into which, I believe, ergot as well as every other new medicine has been forced. The mantle of the men of Athens appears to have fallen upon the medical populace of the present day; their desire to tell or to hear some new thing is only matched by their extraordinary credulity. Let

* Dass der Gebrauch innerer Reizmittel zur Beförderung der Geburt des Kindes unnöthig, fruchtlos und gesunden Frauen sogar schädlich sei; nachgewiesen, &c.

any man introduce a new instrument with a barbarous name, or a medicine, as ergot, which can cure amenorrhoea and menorrhagia, epistaxis and leucorrhœa, the idol (as Dr. Joerg appropriately calls it) will soon have a host of worshippers; and as it must be to many of these, to all intents and purposes, an unknown god, the sacrifices made before it are not difficult to be imagined.

ART. XXII.—*Letter from Dr. WILLIAMS on the Subject of Asphyxia.*

TO THE EDITORS OF THE DUBLIN JOURNAL OF MEDICAL AND
CHEMICAL SCIENCE.

Liverpool, May 12th, 1854.

GENTLEMEN,

As the Editors of a distinguished medical publication, I deem it necessary to address you on a subject that has not only reference to myself, but in some respect to medical literature. Presuming it likely, that the work on the Physiology, &c., of Asphyxia, just published, by Dr. Kay of Manchester, may be reviewed in your Journal, I am desirous of putting you in possession of certain circumstances connected with his subject. I may remark, though familiar to you, that until lately, Bichat's views of the nature and cause of asphyxia were considered the most probable and best established; hence they were those that were taught and admitted in our colleges and medical writings.

In a paper, on the Cause and Effects of an Obstruction of the Blood in the Lungs, published in the 19th volume, 1823, of the Edinburgh Medical and Surgical Journal, I have given an account of a series of experiments which I had performed. From phenomena which were witnessed in these experiments, I deduce, "That the obstruction of the blood in the lungs, on suspension of respiration, is not the effect of a mechanical cause, that is of collapse or subsidence of the lungs; that the obstruc-

tion of the blood in the lungs, on suspension of respiration, arises from a deprivation of pure atmospherical air." From these deductions, and the facts, that an animal can survive only a very limited time the suspension of the function of respiration; and that the blood undergoes a wonderful change in consequence of its being acted upon by the inspired air;—I infer, "That the blood cannot pass from the system of the pulmonary artery into that of the pulmonary veins, without first undergoing those unknown changes from the action of the inspired air."

In my paper, I have noticed, that Harvey supposed the circulation, in asphyxia, to be arrested in the lungs; Goodwyn and Bichat in the heart. That Harvey attributed the vacuity of the aortic system, after death, to an obstruction of the blood in the lungs, in consequence of their collapse. That Goodwyn and Bichat concurred in imagining asphyxia to be immediately dependent upon the circulation of black blood, or blood of a venous character; the left ventricle, according to Goodwyn, ceasing to act "from a defect of a stimulating quality in the blood itself;" the heart ceasing to act, according to Bichat, from the circulation of black blood, (*sang noir*,) in the coronary arteries, this fluid stopping, (*empêche*,) the action of its fibres. The error of these several views I have pointed out, and in conclusion, I have advanced the theory; that the immediate cause of the cessation of the action of the heart, on suspension of respiration, arises from the obstruction to the circulation in the lungs, not from a state of collapse of these organs, as supposed by Harvey, but from a deprivation of pure atmospherical air. Further I have cursorily examined how far the obstacle to the circulation of the blood, from a deficiency of pure atmospherical air, is adequate to explain the cause of some of the vital phenomena observed in health and disease.

My paper, as I have above stated, appeared in the 19th vol., 1823, of the Edinburgh Medical and Surgical Journal. In the 29th vol. 1828, of the same Journal, Dr. Kay published an essay, entitled, *Physiological Experiments and Ob-*

servations on the Cessation of the Contractility of the Heart and Muscles in the Asphyxia of Warm-blooded Animals, in which he develops the same views, with respect to the immediate cause of the cessation of the action of the heart, as I had already done, by a similar or analogous series of experiments. In this essay, no reference is made to my paper which had appeared in the same journal, four years previously. This circumstance I mentioned to Dr. Kay. Dr. Kay, in 1831, published further experiments on suspended animation, in the North of England Medical and Surgical Journal. In this essay, in a marginal note, Dr. Kay says, that I had called his attention to my paper; and states my having anticipated him in one of his conclusions, namely, "That the obstruction of the blood in the lungs, on suspension of respiration, arises from a deprivation of pure atmospherical air." This marginal note is copied into Dr. Kay's recently published volume, on the Physiology, &c. of Asphyxia. However, though Dr. Kay enters very elaborately into the history of asphyxia, yet, no notice whatsoever is taken of my having questioned the correctness of Bichat's hypothesis; nor of my having advanced the theory, that the immediate cause of the cessation of the action of the heart, or asphyxia, on suspension of respiration, is the effect of the circulation being obstructed in the lungs.

I regret that any circumstances should arise to render it imperative on me to make this communication. But, in consequence of the author of the article on asphyxia, in the Cyclopaedia of Medicine, and the reviewer of Dr. Kay's recent work, in the Liverpool Medical Journal, having given Dr. Kay exclusively the credit for being the first who pointed out the error of Bichat's hypothesis, and established, that the immediate cause of asphyxia is the obstruction to the circulation in the lungs, I deem it incumbent on me, in my own defence, to set the profession right on these points.

I am, Gentlemen,

Your very obedient Servant,

DAVID WILLIAMS, M. D.

P. S.—Should the subject of asphyxia be hereafter reviewed in your Journal, I trust the reviewer will take the trouble of examining the papers alluded to, and thereby satisfy himself of the accuracy of this statement, and do justice accordingly.

ART. XXIII.—*Extract of a Letter from Dr. Arrowsmith to Dr. Graves.*

Coventry, 19th April, 1854.

SIR,

In the 12th number you published a paper on some forms of pneumatosis and in particular a case of emphysema following profuse hemorrhage, which at that time was quite a new observation to me. Happening about the same period to consult Portal's *Memoires sur la Nature et le Traitement de plusieurs Maladies*, I was agreeably surprised to find a long memoir on pneumatosis, (or pneumatie, as he prefers to designate it,) in which are contained many interesting observations on the morbid development of air in different textures and under various circumstances. Portal's memoir contains references to numerous writers, chiefly the older ones; and professing as it does to be a somewhat systematic treatise on the subject, I was surprised that he did not notice the observations of J. P. Frank which you have so much, and I doubt not, so justly commended. I have not the work *De Curandis Hominum Morbis* at hand, but since the publication of your paper I have looked into a few works on this subject, without discovering in them any allusion to the detailed memoir of Portal; under these circumstances I have determined to take the liberty of directing your attention to that work, confidently presuming that if it have already fallen under your notice you will readily excuse me for troubling you with this communication. Indeed I am the more induced to

address you from observing that so eminent and learned a pathologist as M. Bouillaud, in the article *Emphyseme*, (*Dict. de Méd. et Chir. Pratiq.*) takes no notice of the writings of either Frank or Portal, and appears to think that the secretion of air can scarcely occur in the human body, at all events that, with the exception of the instances adverted to by Morgagni, where air was found in the cerebral vessels, it is when present due to putrefaction, commencing even in the living body.

M. Rebolle de Gex, I observe, entitles his paper, on a *New Species of Emphysema*, &c.; but one of the first paragraphs in Portal's Essay appears to show that he was perfectly acquainted with the coincidence or dependence of emphysema in hemorrhage, "*La pneumatie*," he says, "*est encore plus fréquente après évacuations sanguines qu'après toute autre.*" "Qu'on parcourt les auteurs qui ont traité de cette maladie, et l'on y trouvera un grand nombre d'exemples. Tout le monde connaît d'ailleurs les pneumaties qui sont la suite de grandes hémorrhagies." v. 107.

Portal quotes cases of pneumatosis from suppressed ptyalism and diabetes, from the retrocession of measles and suppressed catamenia and diarrhoea, numerous examples in fevers, and several instances of the presence of air in the cavity of the pleura combined with fluid effusion, almost always I think from pleuropneumonia; indeed Portal insists much on the connexion of inflammation with pneumatosis. There are cases also of gaseous effusion into the pericardial cavity, and mixed cases of ascites and tympanites. Emphysema of the cellular membrane as a result of inflammation, Portal thinks a not uncommon occurrence, and he quotes Fontana for instances of the development of air in that texture from the bites of venomous reptiles. It is evident indeed that Portal thinks pneumatosis a morbid state which *might* be frequently noticed, and on reading his essay one is struck with the reflection that he must exaggerate its frequency, otherwise practitioners would have been more familiar with it.

The elastic fluid formed in pneumothorax has been analysed

by the late Dr. A. Duncan and others, I think, and the observation of MM. Balley and de Gex on the inflammability of the air, in the cases which they treated, are exceedingly interesting. It is to be hoped, however, that further chemical experiments will be made as opportunities present themselves, for no doubt these gaseous secretions vary a good deal in composition. Thus Portal shews that they are sometimes not inflammable; "*l'air*," says he, "*en sortant se manifeste en faisant un certain bruit; et Ateint même une bougie allumée comme Morand et Senso l'ont observé.*" (p. 131.). On some occasions it is fœtid, in more numerous instances, inodorous. Portal has quoted a case of Daniel Hoffmann of emphysema from fractured rib, in which he appears to imagine that air issued from a vein (with the vein when opened in the operation of venesection. He is, of course, have been mistaken. His words are: "*La veine ayant été ouverte par une saignée pratiquée a ce malade, l'air en sortit en rendant un son comme celui d'un sifflet.*" (p. 186.) The air must, I presume, have issued from the subcutaneous cellular membrane at the same time with the blood, but not out of the vein. Portal appears to have been aware of the fatal or injurious effects of the experimental introduction of air into the veins, and of the occurrence of sudden death from the opening of a vein in the neck. Although I have said that Portal must have been mistaken in his interpretation of the above case, he might ask by what channels does the air effused in emphysema escape if not by the usual course of absorption? I beg pardon for occupying you so long, and am, Sir,

Your respectful and obedient Servant,

R. ARROWSMITH, M. D.

ART. XXIV.—*Case of Constriction of the Aorta with Disease of its Valves, and an Anomalous Tumour in the Right Hypochondrium.* By ROBERT LAW NIXON, A. B., Licentiate of the Royal College of Surgeons in Ireland, Surgeon to Saint George's Dispensary, &c. &c. &c.

La science possède encore bien peu d'observations relatives à l'oblitération et à la constriction complètes de l'aorte.—R. J. BAXTER, M.D., Maître de la Faculté.

THE truth of Bertin's remark, as quoted above, is rendered abundantly evident by the comparatively small number of well authenticated cases of obliteration or constriction of the aorta with which we are acquainted, the total silence observed on the subject by some of our best writers on morbid anatomy, and the absence in many of our most diversified and extensive pathological museums of any preparation to exhibit such lesions; * it is therefore hoped the accompanying case may not prove uninteresting, more particularly as it was attended with some peculiar symptoms not mentioned in those previously recorded, and as abundant opportunities were afforded of observing the progress of disease from the moment it first became manifest.

Before entering upon its detail, it may not be unadvisable to take a brief survey of a few of the most remarkable instances with which we are acquainted of obliteration and constriction of the aorta, as well for the purpose of contrasting them with the present as with each other.

As might *a priori* be concluded from the peculiarly important part which the aorta bears in carrying on the circulation,

* In the museum at Guy's Hospital, I find three preparations exhibiting a more or less perfect obliteration of the aorta immediately above its bifurcation, and in a third instance occurring to Dr. Bright, two points of obliteration were found, one above, the other below the diaphragm, caused by cauliflower-shaped masses of bony matter apparently of rapid development.

an importance increased in proportion to its proximity to the heart, very few instances are afforded of obliteration of its arch even after it has given off the great vessels to the head and superior extremities, and not one before it has accomplished that purpose. Perhaps the most remarkable case on record is that given by Mr. Graham of Glasgow, and inserted in the 5th volume of the "Medico-Chirurgical Transactions;" it occurred in the person of a lad named Henry Frens, aged fourteen, who was admitted into the Glasgow Infirmary in August, 1813, labouring, as was supposed from his symptoms, under pneumonia; he was treated accordingly, and left the hospital in the October following "cured;" he returned again in November with all his symptoms aggravated, and died the following month. Upon examining the body, amongst other phenomena it was found, that, after giving off the branches to the head and superior extremities, the diameter of the aorta was preternaturally contracted, and continued so till its union with the ductus arteriosus, when it became completely impervious; its coats were not thickened or diseased, the obstruction was about a line in breadth, the ductus arteriosus was *impervious*. A case somewhat resembling this, but differing essentially in the fact, that the ductus arteriosus was not only pervious, but of sufficient calibre to admit a catheter, is given in the 33rd vol. (for the year 1815,) of the "Journal de Medecine par Corvisart Leroux et Boyer;" it was here found that six or seven lines below the point where it gives off the left subclavian, the aorta was obliterated for some lines. In Dessault's Journal de Chirurgie, vol. 2nd, p. 108, we find recorded the remarkable case given by M. Paris, demonstrator at the Hotel Dieu, it presented itself in a subject brought in for dissection. After injecting the body the thoracic and superficial abdominal vessels were observed to be tortuous and remarkably enlarged, and, upon further examination the aorta was found to be contracted immediately beyond its arch to the thickness of a writing quill; its coats were natural, nor could anything be found either in its own structure or the neigh-

bowing parts to account for so singular a lesion. It is to be regretted that the state of the heart is not mentioned, nor could any information be gained of the case previous to death. In the first volume of Cooper and Travers' *Surgical Essays*, a case is inserted as having occurred to Mr. Winstone and Sir Astley Cooper. The constriction, as in Mr. Graham's case and others, took place at the point where the aorta is joined by the ductus arteriosus, (which was impervious,) and barely admitted the passage of the little finger; the heart was healthy, and the constriction appeared to depend upon a thickening of the circular fibrous structure of the vessel accompanied with some ossification of its coats. Cases somewhat similar to the above, but occurring more frequently in the abdominal aorta, and in many instances the result of various states of disease, are to be found scattered through the works of different authors. It would be needless here to detail the cases individually, but those who are interested in the subject will find them in Stengel, in his "*Dissertatio de Steatomatibus Aortæ.*" Meckel, (*Memoires de l'Academie Royal de Berlin*, 1756;) Stoerk, (*Annals Med.*, tom. ii. p. 262;) Sandifort, (*Observations Anatomie Patholog.* No. 10;) Morand, where the obliteration was the result of a violent contusion, (*Memoires de l'Academie R. de Paris*, 1736;) Brasdor, (*Recueil Periodique de la Societe de Medecin a Paris*, No. 18;) Velpeau, (*Revue Medical*, tom. iii. p. 326, ann. 1825;) Beynard, (*Journal Hebdominaire de Medicin*, tom. i. p. 161;) Dr. A. Monro, (on Aneurism of the Abdominal Aorta;) and also the very interesting case given by Dr. Goodison, and which is published by Mr. Crampton in the second vol. of the *Dublin Hospital Reports*, page 193, in the year 1813. Dr. Goodison, in examining the dead body of a woman in the Hospice de la Pitié at Paris, discovered a hard tumor placed upon the aorta, and accompanied with an obliteration of that vessel from the origin of the inferior mesenteric artery downwards the remainder of its length. At the obliterated part of the abdominal aorta there was a firm bony sheath covering the

vessel for about two inches, and filled with a hard fleshy substance, which extended further upward, and was firmly adherent to the coat of the artery; it was the inner coat itself which was ossified; the mitral and tricuspid valves were diseased, but the general appearance of the body was not unhealthy, and the lower extremities to all appearance had been sufficiently supplied. This case, of which mention is made in Andral's Pathology, is essentially different from Graham, Paris, and others, where no disease at the point of constriction could be detected. In mentioning these cases I should not omit to add one published in the first number of the North of England Medical and Surgical Journal (August, 1830), by Mr. Joseph Jordan, of Manchester. In this instance the constriction took place about three lines below the union of the ductus arteriosus, (which was impervious), and appears to have completely obliterated the vessel. As in the cases of Paris and Goodisson, a subject brought in for dissection affords this example, and consequently nothing of the previous history could be learned. The pericardium was found distended with blood, which escaped from a small fissure on the right and posterior side of the heart. An abstract of this case is given in the 732nd page of the second volume of the Lancet for the years 1829—1830.

In the subjoined case the constriction resembled that described by Paris and Graham, (no disease presenting at the constricted part of the aorta,) and occurred in an individual affording other and peculiar circumstances connected with the circulating system.

Doctor B., an English medical gentleman, aged 27, placed himself under my care in July, 1833, labouring under dyspepsia of rather an aggravated form, the result, as he supposed, of some alteration which he had made during the preceding month in his usually very temperate habits. He had a delicate, bilious appearance, but stated himself to be naturally of good constitution, but never very robust or strong, and mentioned that even from his earliest days he was subject to occasional attacks of

pain in the right side, accompanied by derangement of the bowels, and other annoyances, which usually subsided in a few days, without leaving any permanent inconvenience. The symptoms when he first sought my advice were, loss of appetite, thirst, foul tongue, disinclination for exercise, slight pain in the right hypochondrium, accompanied by tenderness and fullness of the portion of the liver corresponding to this situation; his bowels were confined and his urine was high coloured. Under the use of purgatives, antimonials, and a small quantity of pil. submuriatis. hydr. comp., aided by strict attention to diet, he rapidly improved; but in the course of a month returned to me again, labouring, as he stated, under hepatitis. Upon examination, I found the liver much enlarged, painful to the touch, and rather harder than natural; he was put under such treatment as his case appeared to demand, but improved slowly. Early in September (about a fortnight after his second attack) I perceived a small tumor situated below the margin of the false ribs, and apparently placed upon the inferior portion of the right lobe of the liver. In a few days after the tumor was observed, I perceived it communicated to the finger a slight pulsation, which increased daily, until at length it became perfectly evident even to the eye; the tumor also continued to enlarge, and at the expiration of three weeks from its first appearance it had attained the size of the half of a small apple. It was accurately circumscribed, and presented both to the feel and by auscultation the peculiar purring thrill of aneurismal varix. The action of the arterial system, so far as it could be examined, was found to increase in strength in proportion as the tumor and its pulsation became more manifest; this was peculiarly evident in the carotids and right subclavian, whose throbbings became at length most unpleasant to my patient.* The aorta exhibited also a remarkably loud *bruit de soufflet* through its whole course, and in addi-

* In Dr. Graham's case, a precisely corresponding state of these vessels was remarked.

tion to his other symptoms, Dr. B. was troubled with considerable dysphagia, having had recourse, without advantage, to leeching and blisters, with the internal exhibition of digitalis purgatives, and a strict attention to diet. I requested the advice of my friend Dr. Wm. Stokes, who met me in consultation in the last week of September (one month after I had first perceived the tumor). Having given as much of the previous history of the case as came within my knowledge, and the detail of the treatment, a most careful and minute examination was made both by the stethoscope and otherwise, and having been frequently repeated during the succeeding days, we came to the opinion,—that inflammation of the lining membrane of the aorta existed, that the liver was enlarged, the enlargement being possibly congenital, at least to a certain degree, and that the pulsating tumour in the hypochondrium was in all probability aneurismal, but from its position, and the peculiar circumstances attending it, we felt great difficulty in determining its seat or actual nature.

In addition to the previous treatment, in which it was agreed our patient should persevere, he was directed a Belladonna plaister to the side, to be worn constantly, and renewed from time to time; and he was also advised to remove from town. He accordingly left Dublin for the country, where I had the opportunity of seeing him as often as he might require. Through the months of October and November, he persevered in the treatment laid down for him by Dr. Stokes, who occasionally saw him, and agreed with me in thinking that matters grew progressively worse, and that a sudden death at no very distant period was the only result to be anticipated, the action of the heart and arterial system becoming daily more violent, and the pulsation in the tumor increasing both in strength and in its progress towards the surface. In the latter part of November he incautiously exposed himself to cold,* and the result

* In Mr. Graham's case a similar cause appeared to produce the fatal symptoms.

was an attack of Bronchitis, attended by considerable dyspnoea. The extreme weakness, the result in a great measure of the necessary medical treatment, combined with the very low diet, under which he had been placed for some months, forbade the abstraction of blood; but other means failing to afford relief, and the symptoms becoming more and more urgent, particularly the cough, which, under existing circumstances, was much to be dreaded, I directed three leeches to be applied over the trachea in the supra-sternal depression, and their bleeding to be carefully watched. Owing, however, to carelessness or want of skill on the part of the nurse-tender, or perhaps in consequence of the uncoagulable condition of the blood, dependent on his strict abstinence, the oozing from the leech-bites was permitted to continue for forty hours before I was again summoned. On my arrival I found my patient perfectly blanched, his pulse scarcely perceptible, his voice feeble, and his strength at its minimum; in fact, he appeared almost moribund; the bleeding from the three orifices was still continuing freely; and from what I saw and could learn, the quantity of blood lost was much more than I conceived my patient possessed; of course my first object was to prevent further hemorrhage, which I readily accomplished by making compression with a graduated compress on the bites, having previously inserted into their orifices, by means of a small blunt probe, a minute portion of the beaver from a black hat, a material I have always found to be of peculiar utility on such occasions.* Placing my hand casually on the situation of the tumor, I perceived it was diminished in size, that its pulsation was much weakened, and it was also observed, as well by my patient as myself, that the distressing throbbing of the vessels in the neck had nearly ceased. These circumstances, however, I attributed to the great loss of blood,

* The black beaver seems to act not only mechanically by its pressure, and also by its filaments delaying the blood as it passes, and thus favouring coagulation, but also somewhat chemically, probably owing to the styptic nature of the different substances used in dyeing it.

by which the power of the circulation generally was so remarkably weakened. Fearing the result of such a sudden and extensive drain upon the diminished strength of my patient, I was induced not only to allow him animal food, in properly regulated quantities, but also to add half a glass of wine two or three times each day. His medical treatment was also materially altered. Quinine, to the extent of two grains, three times in the day, and quarter of a grain of acetate of morphia, being substituted for his digitalis and purgatives.

At one of my visits a few days after, I was agreeably surprised to find that, although the pulsation at the wrist was much stronger, that in the tumor was not proportionably so; and from this period (24th November) daily decreased, until at length it altogether ceased. About ten days after the application of the leeches, its declension was accompanied by a corresponding diminution in the size of the tumor, which continued from day to day to get smaller and harder, until it had assumed somewhat the feel and size of half a marble. Even here it did not rest, but before Christmas had so totally disappeared as to leave no trace whatever of its previous existence, even on the most accurate examination, nor could it ever be detected after this period.

On the 1st of January I was hastily summoned to see my patient, who, I was informed, was dying. On my arrival I found him labouring under spasmodic dyspnoea, accompanied by violent pain under the right scapula and down the side. This appeared to depend upon rheumatism of the intercostals, as it was increased during their action, and pressure on them caused considerable distress; in fact, he had never completely thrown off the attack of bronchitis which he contracted in November, and the present annoyance I was induced to refer partly to this cause and aided by some additional cold. His Quinine was necessarily suspended, his diet reduced, and æther and Morphia given freely; warm turpentine frictions were applied to the side and shoulder, and under this treatment the recent attack was

subdued, and in a few days the lost ground was regained. He was now removed to town, his Quinine, animal food, and wine were restored to him, and their quantity gradually increased. His strength returned, his appetite improved, and he even began to put up flesh, and ceased to be the perfect *anatomie vivant* he was. Exercise in a carriage was now recommended, and used, and at the beginning of February he was able to walk a mile with great comfort, and very inconsiderable fatigue. He, however, felt himself so well, as to make him careless of using the precaution to which he had been so long accustomed; he even went out sometimes in the evening, and on one occasion did so when the weather was very severe; the result was another attack of bronchitis, attended by dry harassing cough, dyspnoea, loss of appetite, superficial excoriation of the fauces, and hoarseness. The plan of treatment found so serviceable in the former nearly similar attack was again put in practice, and with success, though not so rapidly as before. Matters continued stationary for a few days and then began to amend; but a new and most unfavourable symptom now presented itself, namely, anasarca of the lower limbs; for its relief, diuretics, stimulating liniments, warm pediluvia, bandages, horizontal position, &c. &c. were resorted to, and with temporary advantage. The improvement was, however, only of short duration, the anasarcaous effusion gradually progressed towards the trunk, and advanced until it pervaded in a greater or less degree the cellular system of the entire body. The difficulty of breathing was increased most alarmingly, accompanied by ascites and tympanitis; for the relief of the former, diuretics and purgatives of various kinds, and in different combinations, were ineffectually exhibited; the latter was much relieved by the administration through a long tube of a large foetid enema with turpentine. Effusion now took place into the cavity of the chest. The following day a blister was applied with some advantage; the sputa became tinged with blood, and great difficulty was found in their discharge. Matters got worse; he was directed camphor mix-

ture, with Hoffman's anodyne and expectorants; another blister was applied to the chest, and, as in the former instance, with improvement, but for a very short time. At this period an emetic was recommended in consultation, in order to endeavour to act on the overloaded air cells, and improve his powers of respiration by assisting a more free discharge of the mucus which they contained. Accordingly, an ounce of hippo wine was taken with a few hours advantage; the symptoms, however, returned on the succeeding day with increased violence; every attempt even to relieve his distress proved ineffectual, and my patient expired on the morning of the 12th April, retaining his mental faculties in perfection to the last moment; his death, though slow, was still in one sense of the word sudden, as he expired a few seconds after he had spoken to me relative to some pain he felt in the posterior part of the neck.

Post mortem.—Having expressed a great desire to examine the body, with a view to ascertain the nature of the tumor, whose appearance and subsidence I had so anxiously watched, a *post mortem* examination was kindly permitted, which I made the morning after his decease, assisted by my friends, Drs. Cuthbert and M'Dowell, physician and consulting surgeon to Saint George's Dispensary, and Mr. Charles Holmes, apothecary to the institution. I had hoped for the advantage of Dr. Stokes's presence, to whose valuable assistance I was so much indebted during the treatment of this interesting case, but his professional duties prevented him attending at the hour named, and particular circumstances did not permit of delay.

The general appearance of the body was, as might be supposed, indicative of the anasarcaous effusion, which, as I have mentioned, pervaded every portion of the cellular tissue, even that of the forehead and face. The skin was tense, shining, of very peculiar whiteness, and pitting upon pressure. There was a remarkable wasting of the muscular system, and an almost total absence of adipose structure. Upon examining the right hypogastrium, there did not appear any trace of the tumor

through the muscular parietes, but from frequent examination during life I could point out accurately the precise situation where it had presented itself for a period of nearly four months. Having laid open the abdomen by a crucial incision, a quantity of fluid was found in the cavity. The liver immediately presented itself to our notice considerably enlarged, and pressing down between the margins of the ribs and sternum; it was peculiarly hard, and exhibited over its outer surface several large yellow tubercles. On examining that portion which corresponded to the point at which the tumour had presented itself, I was surprized to find no appearance whatever of swelling, enlargement, or even irregularity of surface, connected either with the viscus, or in the abdominal muscles, nor did any adhesion exist between these parts. The liver being cautiously removed and submitted to careful examination, we were not able to discover the slightest trace of tumor in or about it; when handled, the part where the tumor had been supposed to have its place, felt rather harder than the other portions, but upon carefully cutting into it, no cause for such hardness could be found, nor did any trace appear of the remains of an abscess, or of any deposition of lymph-fibrine, or coagulated blood; the tubercles, some of which were evident on the external surface, were very abundant in the internal as to give to the whole organ a yellowish tinge; the enlargement and hardness extended to every part of the viscus; the gall-bladder contained about two ounces of healthy looking bile.

The stomach and intestines were perfectly healthy, the latter particularly diminished in calibre, owing, of course, to his nearly perfect abstinence for some weeks before his death. The colon, however, is to be excepted, as it contained a great quantity of flatus, and thus accounted for the tympanitic symptoms. The omentum presented scarcely a trace of adipose structure, the kidneys were larger and paler than usual, and the ureters appeared to be increased in size and strength. The spleen was large, soft, and distended with peculiarly dark coloured blood;

the pancreas was normal, nor could any alteration be detected in any branch of the coeliac axis; so far then as the abdomen and its contents were concerned, we found nothing to account for the tumor or its pulsation. We next proceeded to examine the thorax, in the cavity of which we found a considerable quantity of water of the usual character. The lungs appeared emphysematous on their surface, and gorged with blood, especially their posterior region. The superior lobe of the left lung was so completely hepatized as to sink when placed in water, and in truth looked more like the structure of a (healthy) liver than the one before us did. Upon cutting into them, their internal structure exhibited, as their external did, an emphysematous appearance; the small air-cells were rendered impervious to air by the effusion of a frothy sanguineous fluid, which the larger contained in great quantities. The trachea also, and its various ramifications, were coated by it, and bore evident marks of inflammation of their lining membrane. The larynx was not examined, but would doubtless have presented similar appearances. The pericardium, when cut into, appeared healthy in structure, but contained a few spoonful of straw-coloured fluid; the heart was larger than natural, the increase depending, as was afterwards proved, upon excentric hypertrophy of the left ventricle. The coronary vessels were dilated and filled with dark coloured blood. The dissection of the heart, which was most carefully made, exhibited the following particulars, of which notes were taken at the moment: The organ (as has been stated) was large and fleshy, exhibiting hypertrophy of the left ventricle with dilatation; the interventricular septum thick and fleshy; the right ventricle smaller than natural, caused apparently by the projection into it of the thickened septum; the right auricle large, the left normal; the auriculo-ventricular openings, on both sides, enlarged; the aorta at its origin was healthy in structure and natural in size. A small abscess, about the size of a pea, was found in the muscular structure of the heart, near the point where it is joined by the

aorta; the art. innominata considerably and the left subclavian slightly increased in calibre; the left carotid normal; all the blood extremely dark in colour, owing to the imperfect oxydation it obtained in consequence of the diseased state of the lungs. The aorta, in its transverse portion, just at the point where it is joined by the ductus arteriosus,† exhibited a very singular constriction, similar to what would occur if a sharp instrument had been pressed upon its upper surface until it had diminished the calibre by about one-half. This will be better understood by reference to the accompanying plate, and the preparation from which it was taken, which is preserved in the museum attached to the Richmond School of Surgery. There was not, however, any deposition in its coats of osseous or calcareous matter at this point, and the ductus arteriosus was pervious.‡ The aorta, in the remainder of its course, appeared perhaps a little narrowed in its calibre, but normal in structure, if we except a few atheromatous spots on its lining membrane; the pulmonary artery, natural. Upon passing in the finger for the purpose of examining the state of the aortic valves, we were surprised to observe that it could not find a passage either from the heart to the aorta, or *vice versa*; but upon laying open the vessel, the difficulty was very easily explained, the valves being, if not removed, at least completely obliterated by an irregular fleshy mass, which grew from the line of junction of the vessel with the heart, and almost totally filled the canal.§ Several par-

* This exactly corresponds with the state of the vessels in the cases detailed by Mr. Graham and M. Paris.

† It is a remarkable fact, that in the cases given by Mr. Graham, M. Paris, Corvisart, Sir A. Cooper, and Mr. Johnston, the point of constriction corresponded almost accurately with that in the present instance.

‡ Except in Corvisart's case, the duct was always found in this state.

§ This growth is described by Corvisart and other French writers under the term "vegetations." Several specimens are now preserved in the different museums, and notice is taken of them by Mr. Wardrop, the editor of Dr. Baillie's works, as the Doctor does not himself allude to them.

ticles of phosphate of lime were deposited in the mass; in particular, one very hard and white portion presented itself, about the size of a small bean. The communication between the heart and aorta was reduced to an irregular slit-like opening, through which a common probe could not pass without some difficulty; and yet, through it all the blood of the system found its way. Here, then, was the seat of disease, for if we except the dropsical symptoms and the diseased state of the lungs, which are to be considered more as effects than causes, and were only of late occurrence, it was, with the morbid state of the aorta, the sole clue which dissection afforded to assist in unravelling the difficulties of this very curious case; and with all the evidence before us, it becomes an inquiry not devoid of interest to consider what share had this morbid growth, and the diseased action of the heart and aorta consequent upon it, in the production of the tumor, whose existence for nearly four months has been already detailed? What was the nature of the tumour, and where situated? to what did it owe its pulsation? If to the action of the enlarged and diseased heart, why should it decline and ultimately disappear, when that organ must have been daily becoming more and more abnormal? If to the aorta, or one of the branches of the coeliac axis, or any enlarged branch of the hepatic artery, why could not the *post mortem* exhibit it? Was it a simple abscess of the liver obtaining pulsation from some neighbouring part? No trace of such a lesion could be detected. Was it the liver pushed out of its natural situation by the enlarged heart, whose impulses it received? This might account for an extensive projection in the right hypochondrium, and a corresponding extent of pulsation, but could scarcely explain a defined tumor, and a pulsation confined merely to its summit, accompanied by the purring thrill already noticed. But even if we receive it as the cause, we would still have to account for this circumstance, why did the tumor and its throbbing gradually decline and ultimately disappear, when it must be manifest the causes of their origin, (if we grant the morbid state

of the heart and aorta were so), from the progress of the disease, were becoming daily more and more likely, at least to continue, if not to increase them. The same difficulty, or perhaps a greater, would be found in endeavouring to explain the cessation of the throbbings in the different great vessels, if we did not conclude that it depended upon the increase of the disease at the mouth of the aorta, and the consequent diminished stream of blood circulating in the vessels.

EXPLANATION OF PLATE.

- a The Aorta.
- b The Pulmonary Artery.
- c The Arteria Innominata.
- d The Left Carotid.
- e The Left Subclavian.
- f The Ductus Arteriosus.
- g The Constriction.

ART. XXV.—*Researches on the Diagnosis and Pathology of Aneurism.* By WILLIAM STOKES, M. D., Member of the Association of the Fellows and Licentiates of the King and Queen's College of Physicians in Ireland, Corresponding Member of the Medico-Chirurgical Society of Berlin, Honorary Member of the Hunterian Society, one of the Physicians to the Meath Hospital and County of Dublin Infirmary, and Lecturer on the Practice of Medicine in the School of Anatomy, Medicine, and Surgery, Park-street, Dublin.

IN the following communication I shall lay before the reader the particulars of several cases of Aneurism, which illustrate some interesting points in the history of the diseases of the great vessels. I shall then make some general observations on Arterial Pathology, and shall have the pleasure of inserting an important communication from Professor Harrison on this subject.

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Case I.—Aneurism of the Hepatic Artery with consequent Obstruction and Distention of the Biliary Ducts.—Jamm-dice — Death by Rupture into the Peritoneal Cavity.

Samuel Mearns, aged 35, a man of regular habits, but who had formerly suffered from apoplexy, was admitted into my wards on the 7th of August, 1835, in a state of complete jaundice. Nine weeks ago, while in the enjoyment of good health, he was attacked with colic, supposed to be catarrhs, which subsided after treatment in about three days. Jaundice, however, soon appeared, attended with constant itching. For the 6 or 7 weeks preceding admission, the patient had been in a state of complete jaundice, and had lost much weight. The first observed symptoms were a yellowish discoloration of the face and sclerotics. Next, the urine became remarkable for its color, and the stools were pale. The jaundice increased, and the patient became more and more emaciated. The jaundice was not attended with any epigastric pain, increased by pressure, nor with any other complaint. Pulse 112.

On admission, the patient was in a state of complete jaundice, the face and sclerotics being of a deep yellow color. The urine was of a deep yellow color, and the stools were pale. The patient was not attended with any epigastric pain, increased by pressure, nor with any other complaint. Pulse 112.

The abdomen appeared normal, but particularly so in the epigastric region, where it was found to be tender to the touch as if much enlarged. The liver was enlarged, and the lower margin was about 2 inches below the costal margin. The right side of the abdomen was a soft uniform and fluctuating mass, and the gall bladder in a state of distension. The liver was enlarged and was tender to pressure.

In this state he continued for nine days, without any change of importance in the local signs or symptoms. He became more and more emaciated, and afterwards a petechial eruption. The jaundice and coloration in the tumor were variable, the jaundice becoming more intense at times. In none of our examinations did we detect any remarkable pulsation of the tumor, nor did the patient complain of any pain. He was questioned, or a manual examination made, and he was found to be in a state of complete jaundice.

CASE I.—*Aneurism of the Hepatic Artery with consequent Obstruction and Distention of the Biliary Ducts.—Jaundice.—Death by Rupture into the Peritoneal Cavity.*

Samuel Meares, ætat. 35, a man of regular habits, but who had formerly suffered from apoplexy, was admitted into my wards on the 7th of August, 1832, in a state of complete jaundice. Nine weeks ago, while in the enjoyment of good health, he was attacked with copious hæmatemesis, which subsided under treatment in about five days, leaving him, however, with impaired appetite and constipated bowels. For these complaints he attended at a dispensary. On the 29th of July, he first observed the legs and arms slightly yellow. The only other remarkable symptom was drowsiness. Next day the nausea increased, he had some pain in the epigastrium, the jaundice was universal, and he had yellow vision.

On admission he had thirst, nausea, and some epigastric pain, increased by pressure; but of this he did not complain much. Fæces and urine affected as in jaundice. Pulse 112, tremulous.

The abdomen appeared generally tumid, but particularly so in the epigastric region, where the left lobe of the liver could be felt as if much enlarged, and stretching towards the left hypochondrium. The right lobe seemed also enlarged, its lower margin extending to the umbilicus. About two inches to the right of this situation I detected a soft pyriform and fluctuating tumour, which I concluded was the gall bladder in a state of distention. The liver felt unequal and was tender to pressure.

In this state he continued for nine days, without any change of importance in the local signs or symptoms. He became covered by a miliary and afterwards a petechial eruption. The pain and tenderness in the tumour were variable, the jaundice persistent. In none of our examinations did we detect any remarkable pulsation of the tumour, nor did the patient complain of pain until he was questioned, or a manual examination made. On the seventh day, though if possible more deeply jaundiced,

he declared that he saw all objects of their proper hue. He had thirst, anorexia, and for some time a morbidly clean and livid tongue.

On the morning of the 17th of August, he sat up in bed for some purpose, grew faintish, leant back, and speedily expired without a struggle, and seemingly without pain.

Dissection.—The body presented the usual appearances of jaundice. On opening the abdomen, the whole of the intestinal convolutions were concealed from view by a layer of soft and recently coagulated blood, which was moulded into the different folds. This was removed and found to fill a quart vessel.

The liver, contrary to expectation, was found rather small, but very prominent, and beneath its thin edge were seen two projecting tumours; one, the gall bladder, enormously distended with bile, and remarkably stretched; the other occurred to the right of the former, and occupied the notch in the anterior edge of the liver. It was of the size of a large orange, roughly coated with cellular membrane; generally adherent, and without fluctuation. It now appeared that the apparent enlargement of the liver was caused by its displacement, its being pushed from behind forwards by these tumours. The second tumour was strongly bound down to the spine, with the pancreas encircling its lower half. I now made a careful examination of the aorta, which was every where healthy, and had no communication with the tumour. Under these circumstances I proceeded to a careful examination of the tumour and liver, in which I was assisted by my colleagues, Messrs. Porter and Collis, and also by Dr. Houston, curator of the Museum of the Royal College of Surgeons. The tumour proved to be an aneurism of the hepatic artery, covered by the capsule of Glisson, and some cellular membrane, and so situated as to press directly on the bile duct. Its interior was flocculent and contained some coagula. The opening in the vessel was well defined, forming an oval slit, and seemed the result of perfectly circumscribed disease.

The portal veins were seen ramifying and dilated on the inner and inferior face of the tumour, which had ruptured by a rent on its antero-inferior surface.

The state of the biliary ducts throughout the liver was most singular. They were enormously dilated up to their termination. The larger ducts could admit a man's thumb; the dilatation continued to the peritoneal surface of the liver, where numerous projections, varying from the size of a walnut to that of a pin's head, were formed apparently by the distention of their ultimate ramifications; these contained bile, and when opened, that fluid was ejected with considerable force; their colour was of a dark green. The substance of the liver was friable and soft, and seemed engorged with bile;* no disease could be detected in any part of the gastro-intestinal surface.

I have placed this case the first in the present selection, as our knowledge of aneurisms of the hepatic artery is as yet extremely limited. I have not been able to find any instance where the symptoms of this affection were observed during life; and, though I confess, that in this case I had no suspicion of the nature of the disease, and consequently that the observation is imperfect, yet the case presents some important points which may hereafter assist us in the diagnosis of the disease.

The patient had enjoyed good health up to the period of the attack of hæmatemesis; he then continued ailing until the supervention of the jaundice, at which time the first decided

* A somewhat similar state of the liver is described by Mr. Lloyd, in the last number of the *Medico-Chirurgical Transactions of London*. In his case the obstruction to the biliary ducts was caused by disease of the pancreas and duodenum. The hepatic and cystic ducts were so enlarged as to resemble a portion of small intestine, and the liver was full of bile. It is remarkable that the hepatic tumour presented the sign of fluctuation during life, which is attributed by Mr. Lloyd, in part at least, to the distended state of the biliary ducts.

symptoms, namely, pain in the epigastric region, with nausea, made their appearance; this was about ten days previous to his admission, and as far as we could collect, this was also the period when the tumefaction was first observed.

I observed to the class that there was something exceedingly obscure in the nature of the case, for the apparent tumefaction of the liver, considering it to be the result of chronic hepatitis, was much greater than could be expected to have occurred in the course of so short a time. I may add that I formed no conjecture as to the cause of the distention of the gall bladder, nor was any stethoscopic examination performed, a circumstance which I now deeply regret. But that there was no manifest pulsation I feel confident, as such could scarcely have escaped us; in our repeated examinations of the tumour; nor, as far as I know, did the patient ever complain of any feeling of pulsation in the part.

It will be seen how completely the dissection accounted for the anomaly as connected with the rapid tumefaction of the liver, which organ was not enlarged as we expected, but displaced by the pressure of the aneurismal tumour. A similar circumstance is recorded by Dr. Beatty, in his account of this remarkable case of aneurism of the abdominal aorta, which he has inserted in the 5th vol. of the Dublin Hospital Reports. In this case, sometime before death, the liver appeared distinctly enlarged, the tumefaction increasing daily in size; the patient died by rupture of the sac into the right pleura. I shall quote the following from Dr. Beatty's account of the dissection: "Before proceeding to the dissection," says Dr. Beatty, "we wished to let some of the gentlemen present, who had not attended him, feel the enlarged liver, but we were much surprised to find that no tumour could be perceived; this, though at the time inexplicable, was afterwards satisfactorily accounted for."

Dr. Beatty having described the healthy state of the liver, with the exception of some indentations corresponding to the

ribcage in its convex surface, and concluded by the following remark : "The apparent increase of size in the liver, in the case of Mr. M., was evidently, caused by the great bulk of the aneurismal tumour pressing it downwards, and forcing its inferior margin beneath the ribs, and the difficulty in dissecting it after death before the body was opened, was the result of the bursting of the sac, by which its size was diminished, and the liver was suffered to resume its original position. The deep indentations, with which the liver was marked, gave evidence of the pressure to which it had been subjected, and may account for the attack of pain in the right side, which so much resembled hepatitis."

One of the most remarkable circumstances in our case, was the slight degree of pain complained of ; in this respect it differs much from many cases of aneurism of the abdominal aorta, in which, as in Dr. Beatty's case, dreadful colics form the prominent symptom. Should future observations establish the comparatively painless character of aneurisms of the hepatic artery ; we may thus arrive at an additional diagnostic. Indeed, when we consider that the hepatic artery, (unlike the abdominal aorta, particularly at its upper portions,) is not bound down by any strong or unyielding tissues, but lies in a capsule of extensible serous membrane, we can understand that its aneurismal enlargement may occur with little or no pain.

The same circumstance too may probably be found to account for the absence of the usual violent pulsations of an aneurismal tumour, at least during the early periods of the case. The violence of impulse, both of the heart and arteries, in health or disease, seems *ceteris paribus* to increase in proportion to the resisting nature of the surrounding parts. I feel little doubt that the violent impulse of the heart in phthisis is, to a certain degree, at least, produced by the circumstance, that the lung has lost much of its elasticity, and thus offers greater resistance to the motions of the heart.

In conclusion it is to be observed, that in aneurism of the hepatic artery, as in this case, and in that of the abdominal

aorta, as in Dr. Bently's case, we have causes, before unobserved, for apparent enlargement, and other diseases of the liver. These facts then must be kept in view in the diagnosis of obscure affections of the liver or gall bladder.

CASE II.—*Aneurism of the Arteria Innominata, with Hemiplegia.—Dysphagia and Stridulous Breathing.—Obliteration of the Right Carotid and Subclavian Arteries,—the Jugular Veins, and Venæ Innominatæ.*

Michael Mears, a shoe-maker, æt. 34, square built and muscular, was admitted into the Meath Hospital, December 29th. The leading symptoms at the time of admission were, cough, difficulty of breathing, and pain in the chest, head, and neck. The night after admission, however, he was attacked with hemiplegia of the left side. He states that some years ago he had syphilis, and has several times since then been mercurialized for what he calls rheumatic pains. Twelve months ago was in Steven's Hospital for one of these attacks, the pain at that time chiefly affecting his shoulders, head, and back. Was cured in about a month by blisters, warm bathing, and purgative medicines. From that time till the present attack had enjoyed what he believed to be good health, although subject to attacks of shortness of breath, particularly after any considerable effort, also to numbness of the right arm.

The present symptoms came on about five weeks ago, after exposure to a strong current of cold air, having sat for several hours at work near to an open window. Before leaving his work he was attacked with pain in the chest, neck, and head, and on the following day these symptoms were followed by a short dry cough, stiffness of neck, sore throat, and some difficulty in swallowing. The pain in the chest, however, was chiefly confined to the upper part of the right side, and was much aggravated on making a deep inspiration. He attended for advice at the dispensary, and was bled, blistered, &c., but finding his disease getting worse, he applied for admission into hospital. Had continued his employment until four days ago. Mr. Power,

the resident pupil, made a hurried examination of the chest with the stethoscope, detected the signs of dry pleuritis under the right clavicle, and had him admitted. The next morning was found in a state of somnolence, and when roused answered the questions put to him sluggishly and in a thick and indistinct voice. The left side was almost wholly deprived of sensation and motion; while the mouth was drawn to the right; and the tongue protruded to the opposite side, the patient not having the power of directing it to the right. Pupil of the affected side (the left) slightly dilated; sensible to the stimulus of light, but less perfectly so than the other. Hearing somewhat impaired. He states that he got out of bed during the night, to go to the night chair; on attempting to return felt giddy and unable to support himself, and was obliged to seize hold of the side of the bed for support; his hands, however, gave way and he fell on the floor. Says he never had an attack of the kind before, but since he was first attacked, (five weeks ago,) has had pain and noise in the head, ringing in the ears, giddiness on stooping, and occasionally flashes of light passing before the eyes; just before he got out of bed he was attacked with slight numbness in the left arm, which gradually increased, and extended to the left leg. Complains of violent pain in the right side and back part of the head and neck, difficulty of swallowing, cough, dyspnoea, and pain in the chest. Cough of a laryngeal character, with scanty, frothy, mucous expectoration; respiration 18; pulse 84, and full in the left wrist, but exceedingly indistinct in the right, nor on a careful examination could any pulsation be detected in the brachial or axillary artery on this side; tongue loaded, with a streak of dark brown fur down the centre; great thirst; costive bowels, and involuntary micturition; temperature of the two sides nearly equal.

The chest sounds well over its whole extent, excepting the sternal extremity of the right clavicle, which sounds decidedly dull. Respiration in the left lung intensely puerile, but exceedingly feeble in the right, both anteriorly and posteriorly, and

without rale. On applying the stethoscope to the sternal extremity of the right clavicle a very loud double pulsation was discovered with a strong impulse, but diminishing in intensity as the heart was approached, the sounds and impulses of which were natural. On pressing the fingers behind the clavicle a small pulsating tumour could be felt in the direction of the arteria innominata. No bruit de soufflet could be detected in any part of the chest.

These symptoms were materially relieved by local bleedings, cold applications to the head and neck, and purgative medicine; but on the 5th of January, all his former symptoms were much aggravated; and he was attacked with diffuse cellular inflammation of the integuments of the neck, extending from the clavicle to the inferior maxilla; the swelling was soft and elastic, with little discoloration of its surface; but extremely tender to the touch, and with distinct crepitation on pressure. The face was livid and swollen, and the superficial veins of the head and neck much engorged, but particularly on the right side, breathing laborious, hurried, and accompanied with a loud tracheal rattle; deglutition difficult and attended with considerable pain; pulse 96; bowels free, but the urine again passed involuntarily. For the relief of these symptoms leeches were applied, followed by spirituous applications to the neck, and the bowels were kept regular with the purgative pills of the house, and enemata. Under this treatment he again obtained temporary relief, for on the 16th January we have the following report:—Rests well, but complains of being very weak. The diffused swelling of the neck and face has disappeared, and the superficial veins of the head and neck are less distended; head-ach less intense, although still violent, (a very prominent symptom since he entered hospital,) pupils of equal size and equally sensible to the stimulus of light. Has a perfect command over the sphincter and bladder. The pain in the right side of the neck continues, and he usually lies with his head bent towards the right shoulder, feeling most ease in that position. No re-

turn of power in the hemiplegic side. The dulness was found to extend over the two internal thirds of the right clavicle, and a finger's breadth below that bone. A distinct but not a defined tumor can now be felt pulsating behind the sternum and sternal extremity of the clavicle, with a stronger impulse than on admission; and the double sound more distinct than over the region of the heart; appetite good, but complains of some difficulty of swallowing.

On the 25th, of January he complained of acute pain between the shoulders, shooting up the back of the neck to the head; pain in the head much worse. The tumor has greatly increased in size, (its pulsations may now be seen at some distance from the patient,) it extends about an inch above the clavicle, and is bounded internally by the mesial line, and externally by the posterior border of the sterno-mastoid muscle.

My period of attendance on the hospital having arrived, I received this patient with the foregoing observations from my colleague, Dr. Graves. I found him complaining of severe pain in the head and neck, which he compared to the sensation of boiling water passing down his neck. The tumor was increased in size, but from a pulpy state of the integuments appeared larger than it perhaps was. The superficial veins of the head and neck, particularly on the right side, were greatly engorged. Respirations 24; pulse 86, soft and regular, but nearly absent in the right arm. Notwithstanding the increase of the tumor there was less dysphagia. Nearly the whole of the right clavicle and two fingers' breadth of the right sub-clavicular region, with the superior third of the sternum sounded completely dull. The impulse and sounds of the tumor as before, and still without bruit de soufflet. The remainder of the right side sounded clear, but the respiratory murmur was exceedingly feeble, while in the whole of the left lung it was intensely puerile.

I determined to try the effect of small repeated bleedings; at the same time supporting the patient's strength by a little solid animal food; I also directed that he should use as little fluids as possible.

The first bleeding was performed on the 28th; six ounces of blood were drawn; he bore the operation well. The head was much relieved. On the next day the expression of countenance was improved, and the turgescence of the veins diminished. This improvement was but temporary. In two days his bad symptoms returned, and the turgescence of the superficial veins became as great as before. The bleeding was repeated, and the use of digitalis, squill, and calomel, which had been ordered on the 28th of January, was continued. Slight relief followed; but the aneurismal tumor continued to increase. He had stridulous breathing, laryngeal cough, and dysphagia; great dyspnoea, with loud rattling in the throat, and the stethoscope detected intense bronchitis in the left lung. On the 3rd the tumor appeared above the clavicle, its stethoscopic phenomena continuing the same. Thus he continued until the 6th, when he had loud tracheal rattle, with increase of dyspnoea and dysphagia, the skin hot and dry, and the pulse 118, regular. On this day the bleeding was repeated, as before, with relief to a certain degree, the stridulous breathing appearing only when the patient coughed.

11th. All the bad symptoms are increased. The dulness now extends over the antero-superior fourth of the right side, the upper third of the sternum, and the sternal fourth of the left clavicle. The tumor above the clavicle is about the size of a hen's egg, but of an irregular, crescentic shape, extending from the fourchette of the sternum to the posterior border of the clavicular insertion of the right sterno-mastoid, and for about two fingers' breadth above the clavicle. No alteration in the stethoscopic phenomena, excepting that well marked pectoriloquy may be heard on applying the stethoscope to the anterior portion of the tumour; intense bronchitis in the left lung.

From this period to the 17th he continued to lose ground; the stridulous breathing became constant, the voice shrill, and he was occasionally aphonic. On this day we found the pulsation had rather diminished, but the tumour was more diffused, and the

dulness of the chest more extensive, occurring over the superior thirds of the right side and sternum, and the internal third of the left clavicle. The remaining portion of the right side, and the whole of the left, excepting the sternal third of the clavicle, sounds clear; *respiration in the right lung exceedingly feeble; in the left, very puerile, and mixed with loud sonorous and mucous rales.* The pain in the head continues. This has been a very prominent symptom since he came into hospital. On the 20th the patient was still worse; there was cedema of the face, particularly of the right side; loud tracheal rattle, stridulous breathing, and occasional aphonia.

The *whole* of the right side now sounds completely dull, and on applying the hand to the inferior portion while the patient spoke, no vibration was communicated. The strong pulsation continued with a loud double sound, both phenomena diminishing in intensity as the stethoscope was approximated to the heart, the impulse and sounds of which continue natural.

I concluded, from the occurrence of this dulness, that a serous effusion had taken place into the right pleura. We observed that during inspiration the right side dilated slightly, but with great collapse of the abdomen; the right side of the face became cedematous, as was also the paralytic arm, while the right arm and lower extremities continued free from cedema. The emaciation of the lower extremities was very remarkable when contrasted with the upper. On the 27th, he was moribund. The tumor had greatly increased, extending as high as the thyroid cartilage, and being bounded on the left side by the sternal portion of the sterno-mastoid muscle, and on the right by the posterior edge of the corresponding muscle. The tumor had pushed the trachea far to the left side, so that the larynx corresponded to a line drawn from the angle of the jaw to the middle third of the left clavicle. He sunk in the course of this day.

Dissection seven Hours after Death.—Head.—Nothing remarkable on the external surface of the dura mater. About

four ounces of fluid escaped on dividing the membranes. Surface of the brain rather paler than usual, but otherwise healthy and firm. The ventricles contained about half an ounce of fluid. The right corpus striatum rather vascular, and apparently somewhat compressed, and situated nearer to the mesial line than usual. On slicing the substance of the brain, an abscess was exposed, situated about the middle portion of the right hemisphere, commencing superior but external to the lateral ventricle, and running downwards and forwards nearly parallel, but having no communication with that cavity. The abscess contained about an ounce of thick white fluid, resembling the substance of the brain, softened and mixed with pus. The surrounding substance of the brain and external border of the corpus striatum appeared rather softer than usual. The remainder of the brain presented nothing remarkable.

Thorax.—Size of the external tumor much diminished. The larynx, trachea, and cesophagus, which, previous to death, were pressed over to the left side, and were situated nearly in a line with the middle third of the clavicle, are now more in a line with the sternal extremity of that bone. On dividing the integuments, the cellular tissue of the neck and upper portion of the thorax were found infiltrated with a yellow, gelatinous fluid, the muscles pale and soft, and the cervical fascia much thickened. The right lung was found collapsed, and reduced to a dark carbonised mass, in some measure resembling the lungs of the fœtus. Strong adhesions between the two pleuræ, but evidently not of recent formation, the pleura costalis being in some places strong, opaque, and nearly the eighth of an inch thick. The adhesions, however, were not complete over the surface of the lung, but formed circumscribed sacs. These sacs were filled with a straw-coloured serum, some containing from two to four ounces. The left lung was very voluminous, and had formed some slight adhesions to the costal pleura; it appeared somewhat hypertrophied, and firmer than natural; its vessels somewhat engorged, and the tubes filled with mucus. About

two ounces of straw-coloured serum were found in the pericardium. The heart presented on its anterior surface a yellowish brown appearance, but was otherwise healthy. On slitting open the aorta, it was found considerably diseased, somewhat dilated, and the coats thickened; the internal one uneven and nodulated, of a dull light red, speckled with yellow spots. This state was most remarkable in the arch, but did not disappear from the whole of the thoracic aorta. The aneurism was one of the arteria innominata, the whole of the front of which artery was destroyed, and replaced by the wall of the sac; the posterior half, on the contrary, from its origin to its division, was sound. The aneurism arose at first narrow, (two inches in circumference), but gradually increasing in bulk so as to equal a large cocoa nut, the walls being of irregular thickness, in some parts scarcely thicker than paper. The finger, passed from the aorta through the opening of the innominata into the sac, could feel the large fibrous and laminated coagula, with which it was in a great part filled. By its pressure it had so flattened the right side of the trachea that the free margins of the cartilages overlapped those of the opposite side, and almost prevented the passage of air. The right carotid and jugular vein on the posterior surface of the sac were flattened and obliterated, and contained firm coagula. The vagus nerve also was flattened, and its fibres evident and vascular. Both right and left venæ innominatæ were flattened and completely obstructed on the anterior surface of the tumor. The left carotid and subclavian arteries were unaffected, but the right subclavian was pressed so flat at its immediate origin, that no blood could have passed from the innominata, though there appeared to have been a reflux current from anastomosis, as the artery was gaping immediately after its origin, and quite healthy. From the pressure of the tumor downwards, the apex of the lung was much compressed and flattened.

I have been induced to give this case at considerable length, as it illustrates so well the local, general, and stethoscopic phe-

nomina, of an aneurism of the arteria innominata, and as accurate stethoscopic observations of this disease are still desiderata in pathological medicine. The complication, too, with an affection of the brain gives the case an additional interest.

I shall now analyze briefly the symptoms of this case, and connect them with the necroscopic appearances. It will be recollected that for some time previous to the commencement of his last illness he had been subject to attacks of hurried breathing after any considerable effort, and also a feeling of numbness in the right arm. His last illness came on after exposure to cold, when he had pain in the head, neck, and upper part of the right side, which last was aggravated upon any deep inspiration. He next got a short dry cough, stiffness of the neck, sore throat, and a certain degree of dysphagia. It is difficult to say whether at this early period the dysphagia was produced by the state of the fauces or the aneurismal disease. It is generally taught that dysphagia, as a result of aneurism of the aorta, or its immediate branches, occurs only when the tumor has attained a considerable size. But a very small aneurism of the aorta may cause dysphagia, of which I had a remarkable instance some time ago in a case which has been published by my friend and colleague, Mr. Porter.* How far dysphagia from a small aneurism of the innominata, might occur, is yet to be determined. The situation of the vessel would render this less likely than in the case of the aorta. Under these circumstances, it is difficult to determine to which cause we are to attribute the early dysphagia in this case.

How far the co-existence of stridulous breathing with dysphagia may be found to be a diagnostic between aneurisms of the innominata or aorta in their early stage, is an interesting and important question. It seems not improbable that in the for-

* See Dublin Medical Journal, vol. 3, p. 100.

never case the co-existence of these two circumstances would be more constant, the first pressure of the aneurism of the innominate being exercised upon the trachea.

It will be recollected that this patient had cough, and complained of acute pain in the upper part of the right side of the chest beneath the clavicle. Did this depend on the disease of the innominate? It was observed that this pain was increased by pressure, and by the patient taking a deep inspiration, which circumstances, connected with the discovery of frottement in the sub-clavicular region, all tended to shew that the pain, in part at least, was produced by pleuritic inflammation of the upper portion of the right lung, a diagnosis which was made by Mr. Power, who is an accurate stethoscopist and successful cultivator of pathological medicine, and fully verified by the *post mortem* appearances.

I may remark here that I had been for some time doubtful as to the applicability of the test of frottement to the discovery of dry pleuritis in the upper portions of the lung, but I have now had so many cases as to leave no doubt that even in this situation the motions of the pleura are sufficiently extensive to cause the phenomena in question.

We have next to consider the paralytic attack which supervened on the night after his admission. It will be observed that on the next day the patient had violent pain in the head, with a varicose state of the veins of the neck. The sudden occurrence of this paralysis, in a patient labouring under a mechanical obstruction to the cerebral circulation, led at first to the idea that its cause was an apoplectic effusion; further consideration, however, induced us to doubt this opinion: the paralysis came on before the signs of venous congestion were well established; it was accompanied and preceded for some time by violent pain in the head; and the patient stated to us, that for some time previous to his last illness, he had pain in the head, vertigo, tinnitus aurium, and flashes of light in the eyes, with occasional numbness of the left arm and leg. Here then, pre-

ceding the paralysis, were symptoms of a cerebral affection; the two circumstances in favour of apoplexy, being the suddenness of the attack, and the existence of mechanical obstruction to the circulation. Now, as to the first, it seems proved that suddenness of paralysis as a sign of apoplexy, cannot be depended on, unless where no premonitory symptoms of a local affection of the brain have existed; for we know that many cases of circumscribed abscess or softenings of the brain have occurred with a sudden paralysis. In the next place, though venous congestion may produce a circumscribed effusion, yet this lesion seems more generally combined with an affection of the arterial than of the venous system. In this case the cerebral symptoms and their history, accurately corresponded with the lesion of the brain found on dissection.

Let us now consider the symptoms as connected with the chest. One of the most remarkable of these, was the difference of the intensity of respiration in the two lungs, the sound of percussion of which was every where clear, except in the immediate situation of the aneurism. This circumstance, arising from the compression of one bronchus, seems to me a most important one in the diagnosis of thoracic aneurisms, and one to which sufficient attention has not been paid. In the case to which I before alluded, published by Mr. Porter, this phenomenon was well marked, and was in truth the only unequivocal sign of any tumour existing in the chest: and there is at present a patient in the hospital labouring under many symptoms of aneurism of the aorta, though no external tumour is perceptible, and who presents this sign in a most remarkable manner. When we consider the difficulty that has hitherto been attendant on the diagnosis of thoracic aneurisms, *previous to the appearance of tumour*, any additional sign must be considered as of great value. I have no doubt that this one will in many cases prove highly valuable. The observation too, may to a certain degree apply to the diagnosis between chronic laryngitis and the pressure of tumours external to the tube; for if, previous to the co-

currents of the stridulous breathing, this difference of respiration has been observed in the lungs, a difference inexplicable by any result of percussion or auscultation of the respiratory murmur; it will give almost a certainty that the symptoms are not the result of original laryngeal or tracheal disease, but of gradually increasing pressure, first upon one bronchus, and afterwards on the trachea itself.

In this case the phenomena, as connected with the aneurism, were repeatedly and accurately observed, and the gradual extension of the tumour was measured with the greatest facility, by the application of percussion. The aneurism presented all through a double sound, perfectly analogous to that produced by an excited heart, and without any *bruit de soufflet* or *rape*. This double pulsation of aneurismal tumours has been already noticed by several writers, and by some has been thought to arise from the contact of the aneurismal tumour with the heart itself, by which it receives its double impulse, an explanation evidently inapplicable in the present case, as also in others where the tumour has been at a still greater distance from the heart, and totally unconnected with it. The occurrence of double pulsations similar to those of the heart, seems to prove that the action of a single cavity, may, under certain circumstances, produce a double sound, and hence it appears probable, that the division of the heart into auricle and ventricle is not necessary for its double sound. The explanation of the sounds of the heart, founded on this division, must be reconsidered, and it is not unlikely that mere systole and diastole of a single cavity, or of two cavities, acting synchronously, as in the case of the ventricles, with the entrance and exit of a fluid, are all that are necessary for the production of the double sound.

There is a great deal of misconception abroad with respect to the occurrence of *bruit de soufflet*, or other murmurs, as a sign of aneurisms; the truth is, that these phenomena are frequently altogether absent, and we are in great want of facts to explain their presence or absence in particular cases. I have

observed their absence in aneurisms of all parts of the aorta. It has been supposed that the existence of pressure on the tumour may be a cause, yet, in this case, although the tumour was evidently under considerable pressure; no dislocation of the clavicle having ever occurred, though the aneurism rose up considerably, and pushed the larynx and trachea far to the left side; the sign was absent all through. It seems probable that the cause of these phenomena, will be found to reside more in the state of the vessel at its distal or its cardiac orifice, than in the mere existence of the aneurism itself, just as the murmurs of the heart depend so commonly on the state of its orifices.

The last observation which I shall make on this case, relates to the occurrence of the extensive dullness of the right side, which occurred some days before death. It was almost certain, from the absence of the stethoscopic phenomena of pneumonia, that this was the result of a liquid effusion into the cavity of the pleura; and its having occurred when the patient was in a tuberculous state, seemed to shew that it arose either from a hydrothorax, or an effusion of blood from the aneurismal tumour itself. It might be argued against the latter opinion, that an effusion of blood to such an extent, must have produced instantaneous death; but sudden death from the rupture of aneurisms depends more on the mode than on the quantity of effusion. I have already illustrated of this in the fourth number of this Journal, quantities of blood, more than sufficient to have produced and absence of respiration over one lung, over the whole of the body, and yet the patient continued to live. In these cases, both of abdominal aneurism, the blood was effused behind the peritoneum, which it gradually detached from its subjacent tissues, so that it was all through under the influence of pressure; and in one of the patients, after these effusions had been formed and recognized, sudden death took place by another opening of the aneurism into the cavity of the pleura, which was free from adhesions, and so permitted a sudden interruption of blood. Now in the case before us, a morbidous

adherent state of the pleura was to be expected, yet I determined against the opinion that an effusion of blood had taken place, from the force of the pulsations, and the volume of the tumour not having been diminished; and consequently came to the conclusion, which was verified by dissection, that a cadaveric hydrothorax had taken place.

Case III. Dilatation of the ascending Aorta.—Death by Rupture into the Pericardium.

Thomas Stafford, ætat. 50, was admitted into our wards on the 18th of January, 1831. This patient, in the September preceding, in consequence of a fall on his right side, experienced for some time much pain in that situation; in about a month, however, he became affected with a sharp pain in the superior portion of the left side; this continued for about four weeks, when he perceived a pulsation between the cartilages of the second and third ribs, and from this time the pain in the chest considerably diminished. On admission a flattish tumor was found to exist, extending from the second to below the third rib; this presented a double pulsation, closely resembling those produced by the action of an excited heart, and without any *bruit de soufflet* or *rape*; he had no dyspnoea in the erect position, yet when he lay down he was immediately attacked with difficult respiration. The stethoscopic phenomena of the heart and lungs were apparently natural. He was placed on a low diet, bled generally and locally, and the infusion of digitalis prescribed. Under this treatment he improved until the latter end of February; the double pulsation as observed by the hand disappeared, but was always perceptible by auscultation. On the 23rd of February he was attacked with severe pain in the chest, and numbness of the left arm, he could lie only on the right side; by local bleeding and blistering some relief was given, but he could not remain in the horizontal posture for any length of time, from the exacerbation of the pain thus produced. The double pulsation could be again plainly felt. On the 26th he was bled from the arm, with immediate relief

of the pain in the chest and shoulder, and the numbness of the arm; he could now lie in the horizontal position; thus he continued without any important change until the eleventh of April, when, while sitting in bed, and talking to the other patients, he suddenly fell back, and immediately expired.

Dissection.—Body emaciated, no oedema; the ascending aorta was found greatly dilated, the dilatation commencing at the semi-lunar valves, and terminating at the origin of the innominate. The tumour was found adhering to the left side of the sternum, and the cartilages of the second and third ribs, and was about the size of a goose egg. This had ruptured into the pericardium, by a rent of about half an inch in the anterior portion of the sac; the pericardium was distended with bloody serum, and on being opened, displayed a vast coagulum of blood, occupying its posterior and inferior portions; no concentric laminae existed in the aneurismal tumour; the lining membrane of the thoracic aorta was studded with bony depositions; the heart was in all respects healthy, and the lungs perfectly crepitating.

In this instance we have an example of aneurismal disease of the aorta, (simple dilatation,) in which the phenomenon of double pulsation was observed, and in which no *bruit de soufflet* or *rape* existed at any period while the patient was under observation. Here, from the situation of the disease, it might be supposed that the double pulsation was communicated from the heart itself, but we have already seen that this sign may occur under circumstances where the above explanation is inapplicable. I may also observe, that this case, like the last, is illustrative of the accuracy of stethoscopic diagnosis, as relating to the state of the heart, as well as that of the aorta: in both instances, the stethoscopic phenomena of the heart, were natural, and the organ was found free from disease.

It may be an interesting subject for inquiry to ascertain, how far the volume of the coagulum found in the pericardium, might be considered as a measure of the diastole of the heart. At least

a pound of blood existed in the sac: the suddenness of the accident, and the immediate death of the patient, are opposed to the idea that any gradual extension of the pericardium could have taken place, at least to any extent. Under these circumstances, may we not conclude that that portion of space, if we be permitted to use the expression, which would be occupied by the heart in a state of distention, was filled by the sanguineous effusion; which thus may be supposed to equal the contents of the heart, and any increase of volume which it may have during its actions.

The next case I shall detail is an example of thoracic aneurism, in which the sign of inequality of respiration was distinctly observed.

CASE IV.—*Aneurism of the Thoracic Aorta, without external Tumour; recognized during Life.*

Patrick Walsh, ætat. 26, was admitted into my wards on the 23rd of July, 1832, complaining of cough and dyspnoea. He had been subject for years to some cough, but this symptom had been severely aggravated during the last ten days.

This patient was sent in as an ordinary case of bronchitis, but I was at once struck with the peculiar appearance of the neck. This was generally enlarged, (giving the idea of the patient's wearing a collar or tippet,) the jugular veins were turgid and tortuous. No sign of œdema of the extremities, or of abdominal effusion, existed.

He complained of short cough, with frothy, mucous expectoration, and of a stinging pain of the right shoulder, shooting down to the mamma and frequently catching his breath. The cough and dyspnoea were always worse at night; and his sleep was frequently broken by frightful dreams. Pulse regular, 100, rather smaller in the right arm.

The heart's impulse was but slightly increased, yet its sounds were heard loudly over a large portion of the chest; on the left side some bruit de rape accompanied the first sound. The

anterior portion of the chest was less sonorous than natural, especially the right sub-clavicular region, which was decidedly dull. Posteriorly the chest sounded better. In the right sub-clavicular region, the respiration was feeble, with a slight mucous rale; in the right axromial region, the respiration had a tracheal character, while in the left lung the respiration was puerile. On laying one hand over the right scapula, and the other under the clavicle, a distant though distinct impulse could be felt, apparently synchronous with that of the radial artery. No bruit de soufflet audible either under the clavicles or in the axromial region. The patient had some dysphagia which dated from the commencement of the tumefaction of the neck.

Connecting the existing symptoms with the history of the case and the stethoscopic phenomena, I came to the conclusion that this patient in all probability laboured under an aneurism of the arch of the aorta. The patient was treated by small general and local bleedings, and in a few days was so much improved that he requested to be discharged, declaring himself perfectly well. Before he left the hospital, I gave him a strong warning against any excess, stating that it might be followed by immediate death. A short time after this, his former symptoms returned. He entered another hospital in this city, where after a few days he suddenly died while in the act of conversing with some of the patients. On dissection an aneurism of the arch of the aorta, which had produced death by rupture into the pleura, was discovered.

The circumstance which first drew my attention particularly to this case was the peculiar appearance of the neck. Here two phenomena were observable; one, the distended state of the jugular veins; the other, the peculiar tumefaction of the neck, giving the collar or tippet-like appearance to which I before alluded. It is to the latter of these that I wish principally to call the attention of my readers, inasmuch as it seems proba-

ble to me that this appearance will be found a means of diagnosis between dropsy from pulmonary disease, and the tumefaction arising from immediate obstruction of the vessels of the neck. In this case, although the neck was so much enlarged, yet it could scarcely be said to be cedematous, and there was no proportion between the swelling of the neck and that of the face; indeed, after the patient had been treated in hospital, any tumefaction of the face which existed on his admission, which was even then very slight, completely disappeared.

From the feel and appearance of this tumefaction, its circumscription, and occurrence without dropsical effusions in other situations, I would look on it more as produced by a generally distended state of the veins than by effusion into the cellular membrane. In the case of Mearns, this tumefaction occurred first at the side corresponding to the aneurism of the innominate, while in the case before us it was equal on both.

I shall not dwell long on the dilatation of the external jugulars which was observed in this case and in that of Mearns. In the case of Stafford, the tippet-like tumefaction continued after the swelling of the veins had subsided.

Can we, from any character of this state of the jugulars, arrive at a knowledge of the seat, extent, or nature of the disease which is a cause of the distention. Reynaud has already shewn that the existence of an external collateral circulation, by which the veins of the head anastomose freely with the intercostal, mammary, and other superficial veins, may be considered as leading to the diagnosis of obliteration of the superior cava, while the same condition of the epigastric and other external abdominal veins, points out obliteration of the inferior cava, or of the vena porta.*

* See Journal Hebdomadaire; Dr. Graves on the Treatment of various Diseases, Dublin Journal of Medical and Chemical Science, No. XII; Wright on Cardiac Pathology, Philadelphia Journal.

In the case of Mears the distension and tortuosity of the jugular vein was first observed on the side where the tumour existed, and afterwards, as it increased, the opposite jugular vein became nearly similarly circumstanced. The state of the veins innominata will sufficiently explain this. I recollect a case of aneurism which was supposed to be of the subclavian artery, yet in which this state of the veins on both sides existed during life. The result of this case showed that the innominata was engaged, so that it would appear, that in a case of supposed subclavian aneurism on the right side, should this varicose state of the jugular veins on both sides occur, a strong argument against surgical interference would exist, as there would be almost a certainty either, that that subclavian aneurism was of great size, so as to compress both the innominata, or that the disease was more deeply seated, involving the aorta or innominata.

One of the most remarkable circumstances connected with the symptoms and signs produced by the pressure of aneurismal tumours on surrounding parts, is their remarkable variation, at least in the earlier periods. This applies to the stridulous breathing, the aphonia, the dysphagia, and the distention of the veins. I want observations to prove that it also applies to the partial feebleness of respiration; but reasoning from analogy, we have no reason to doubt the fact. This arises, in part at least, from the varying size of these tumours, and the power of adaptation in surrounding parts; and we might inquire, how far in this circumstance of variation a diagnosis between aneurismal and other organic tumours might be established. All cases of solid intra-thoracic tumours should be carefully observed with this view.

It will be observed, that in this case, the sign of inequality of respiration in the lungs occurred in a remarkable manner: to this subject I have already alluded in my observations on the case of Mears; and I shall now return to it as it

seems to me an important addition to our means of diagnosis of aneurisms of the thoracic vessels. When an aneurismal tumour compresses either bronchus, we observe a remarkable feebleness of respiration on the affected side; this feebleness is general, when the main bronchus is compressed, but I believe that it may be found more remarkable in the upper or the lower lobes, according to circumstances. Co-existing with this feebleness, we have more or less puerility of respiration in the opposite lung; while the sound on percussion in the early period, at least, remains the same, and auscultation can detect no sign as connected with the state of the pulmonary cells or pleura, to account for the phenomenon in question. In the early periods we have still some respiratory murmur; but as the tumour increases in size, this becomes extinguished, and we have a tracheal respiration and dulness of sound in proportion to the extent of the tumour, and at this period the other signs and symptoms of aneurism are commonly developed. I do not put this observation forward as leading us to any certain diagnosis of intra-thoracic aneurisms; like all other morbid phenomena it has no value, except when connected with other circumstances; indeed, we may say, that in medicine there is no one sign or symptom, which, taken alone, can be considered as pathognomonic. We know that other circumstances, besides aneurisms, will cause an inequality of respiration in the two lungs, without difference in the sound on percussion. Thus in the Clinique Medicale of Andral, "*Maladies de Poitrine*," we read of a patient who presented a loud respiration in the left lung, while that in the superior portion of the right was much more feeble; percussion on both sides giving the same results. The patient stated that for a length of time, he had experienced a kind of constriction above the right breast, and declared that he did not breathe with the right side of his chest; this patient presented the phenomena of organic disease of the heart, and died with symptoms of hydrothorax. Upon dissection, the superior lobe of the right lung, though little

crepitating seemed otherwise healthy, but its principal bronchus, at a few lines from its origin, was so contracted as hardly to admit the passage of a fine probe. This contraction was owing to thickening of the mucous membrane, and explained the partial feebleness of respiration. The heart was in a state of active aneurism, and the aorta narrowed.

In the same volume, a case is recorded of a patient who laboured under chronic bronchitis, with copious puriform expectoration, the respiratory murmur over the left lung was very intense, while on the right it was much more feeble, the sound on percussion being equal. On dissection, the root of the right lung was occupied by an enormous mass of melanosis, which seemed to have arisen from the bronchial glands, which were so involved in the tumour, as to be undiscoverable; the right bronchus was comprised in this mass, and so compressed, that its calibre was not more than half that of the bronchus of the other lung.

The other causes which might produce the sign under consideration, are the existence of a foreign body in one bronchus, and a partial emphysema; but, notwithstanding that the sign of inequality of respiration, without dulness of sound to account for it, may arise from these several causes; it is yet plain that its existence in any suspected case of aneurism of the aorta, will be an important circumstance, and that in any case where it does occur, we must, in seeking for its explanation, take the possibility of aneurism into consideration; and let it be recollected, that of all the causes competent for its production, this is the most frequent. It is obvious that the history of the case, and the period of duration of the symptom, will be of the greatest consequence in determining this question.

Although cases of aneurism of the abdominal aorta, in which the nature of the disease was not suspected before death, have been described by many authors, still it is of importance to add to our stock of knowledge on this subject. For the notes of the two following cases, I am indebted to my friend Dr. Frazer, a gentleman of great experience in military surgery.

"CASE V.—A native of India, in the pioneer corps of Ceylon, employed as a labourer in road-making, had an accession of symptoms of severe colic in the year 1823. The same symptoms having several times previously compelled him to abandon his work, and to seek refuge in an hospital. Treatment and short repose enabled him to resume his occupation. On the day of his last admission, the urgency of the symptoms was relieved, but on that subsequent, an excruciating recurrence of them took place, and in about half an hour ended suddenly in death.

"Dissection, three or four hours after death, showed a great effusion of blood in the belly, the intestines being in fact deluged with the quantity; it proceeded from the bursting of an aneurism, the size of an orange, situated in the abdominal aorta. The real disease in this case was never even suspected.

"CASE VI.—A British soldier came into hospital, complaining of violent pain in his back, particularly along his spine, no accompanying morbid signs, either local or constitutional, being indicated. His health was good, the natural and vital functions seemed to be regularly performed: there was no emaciation. Leeches, and I believe some other remedies, were used during his stay in hospital; on his dismissal he acknowledged himself relieved. On attentive consideration of the case, suspicion lurked in the minds of the medical attendants, that the case was either feigned or was unimportant; symptoms both local and general being still absent, when he returned a second time, asserting the aggravation of his complaint. After some residence in hospital, he was again discharged without any conclusion having been arrived at as to the nature of the case. The two medical attendants (both experienced in the service) were perplexed, though inclined to believe that the man fabricated much. On his last return to the hospital, he was met by one of the above gentlemen, and to the question, what was the matter with him, the invalid replied, "Oh, sir, I can bear it no longer, I must come to hospital." In about three or four hours this

gentleman was shocked and confounded by hearing of the death of the patient.

"On examination a large aneurism of the abdominal aorta was found ruptured, the bodies of the adjoining vertebrae wasted carious. The surgeon of the regiment to which this man belonged, was a cautious person, and fortunately for himself, though he suspected the reality of the disease, he adopted measures which the false conclusion, that the patient's condition was an impostor, might have led him to adopt.

"CASE VII.—*Aneurism of the Abdominal Aorta opening into the left pleura.*

"A gentleman, aged about 30, who had previously enjoyed good health, was attacked in the latter end of the year 1836, with symptoms of deranged bowels, followed by obstinate and permanent constipation, for which he used much purgative medicine with but little relief; he then began to complain of a sensation of uneasiness and oppression referred to the left side, and a feeling as of a tumour in the left hypochondrium extending to the groin; he had also weakness and pain in the back. The disease was first considered to be an affection of the intestinal canal, then psoas abscess, and lastly disease of the kidney. These distressing symptoms increased, and about three months before he consulted me, a pulsating tumour appeared in the postero-inferior portion of the left side, which soon became of great size. The disease was now treated as aneurismal, and the patient placed on an exceedingly low regimen, but his sufferings from pain and oppression continued to increase, and were frequently so intolerable as to make him long for death. On one occasion, in a fit of despair, he entered a tavern and ordered a sumptuous dinner, of which he partook heartily; he drank a pint of wine and two tumblers of brandy punch; this was followed by an immediate cessation of his distressing symptoms, and he continued quite free from pain for many days, and enjoyed a state of comfort to which he had been a stranger for months. From this time he generally lived well, as he uniform-

ly, found that any return to low diet brought on an increase of his distressing symptoms.

When he came under my care he was pale, emaciated, and had remarkable expression of countenance, indicative of suffering and despair; he referred all his sufferings to the left hypochondrium, where he declared there was a large tumour, which, however, after the most repeated and careful examinations, I was unable to detect, although the part had a certain degree of fulness. Posteriorly, from the short ribs to nearly the apex of the scapula, a vast pulsating tumour existed, the pulsations of which were so violent, and the coverings apparently so thin, that I was in daily apprehension of his sudden death while under examination. Notwithstanding this, the action of the heart was feeble, and although the patient used to walk to my house, (upwards of a mile and a half,) yet on his arrival his pulse or respiration was scarcely accelerated, nor his sufferings in any way increased. About a fortnight before his death he laboured under some excitement, with considerable increase of oppression, and I ventured on taking about six ounces of blood from the arm. The blood was poor and watery, and he bore the operation badly. Soon after this the belly became swollen, the oppression dreadful, the countenance assumed the hippocratic expression, and for the week previous to his death, he laboured under atrocious colics. During this period I detected for the first time a diffused bellows' sound in the left hypochondrium, very similar to that of the placental murmur, and on the day previous to his decease a pulsating tumour could be felt in this situation.

Dissection.—The body was emaciated, with large ecchymoses on the back; an enormous fluctuating tumour extended from below the left scapula to the last rib, the left side was considerably dilated, the intercostal spaces obliterated, and the sound on percussion completely dull; belly tympanitic.

The left pleura was filled with a vast coagulum of blood, and its separated serum; this had almost completely com-

pressed the lung, which did not descend below the third rib; the heart lay at the right side of the sternum. On removing the coagulum we observed that on its upper portion some separation of fibrine had occurred. The pleura was found red, and generally covered with a thin layer of lymph. In the lowest portion of the pleural cavity we perceived the ruptured aneurismal sac displaying its concentric layers of coagulable lymph, which hung in irregular masses in the cavity of the pleura; the sac was found to be of great size, extending inwards and downwards until it reached nearly on a level with the crest of the ilium, its chief protuberance being to the side of the abdomen; five of the vertebræ were deeply eroded, and two of the left transverse processes were found completely detached and enveloped in the coagula. The intervertebral cartilages of most perfectly sound, and were of a pearly whiteness; the left crus of the diaphragm was destroyed and the muscle partly enveloped the tumour in the form of an arch; no effusion of blood into the sub-peritoneal tissue on the lateral or anterior portions of the parietes had occurred, nor were the psoas or iliac muscles engaged. The heart was exceedingly flabby, shrivelled, and of a livid hue, an appearance strongly contrasting with that of the external muscles, which were red and firm; the aorta seemed perfectly healthy until about three inches from its bifurcation, where a well defined oval perforation of the posterior surface existed, which communicated directly with the sac.

I have inserted this case principally with the view of drawing the attention of medical men to the investigation as to how far the treatment of Valsalva is applicable to aneurisms of the aorta.

The case before us exemplifies what has been observed in many other instances, that a low regimen seems to increase the sufferings and accelerate the progress of the disease in patients labouring under aortic aneurisms. On this subject Bertin makes the following remarks:—"Valsalva et Albertini au rapport de Morgagni, ont obtenu la guérison d'un grand nombre d'anév-

riames tant internes qu'externes, par l'emploi rigoureux du traitement qui porte leur nom, Morgagni lui-même, Lancisi, Guattani, Sabatier, MM. Pelletan, Corvisart, Hodgson, Laennec, &c. &c., recommandent cette méthode et citent des faits en sa faveur. Nous partageons leur opinion à cet égard, mais nous admettons que s'il est bien démontré qu'elle a été suivie des plus heureux succès dans le traitement des anévrismes externes, il n'est pas aussi rigoureusement prouvé qu'elle ait réussi le plus souvent dans celui des anévrismes de l'Aorte bien reconnus ~~spécialement~~, et nous pensons que la plupart des cas rapportés par les auteurs sont des exemples de guérison d'aortite pure et ~~simples~~ ou d'anévrismes simulés.

In the case recorded by Dr. Beatty in the 5th vol. of the Dublin Hospital Reports, great alleviation of the distressing symptoms followed the change from a low regimen to a more generous diet. The same circumstance was observed in Dr. Proudford's case, published in the Edinburgh Medical and Surgical Journal, and in the Report of the Meath Hospital, by Dr. Graves and myself; where we suggest that this circumstance may be explained by supposing that in consequence of the use of nutritious diet a more coagulable blood is formed, and thus the extension of the disease for the time suspended. I have myself seen within a short time three cases where the most marked relief followed the change from a low to a generous diet. It is plain that the question is still an open one, but that in the treatment of any case of aortic aneurism, two circumstances must be kept in view, one the degree of excitement of the heart itself, and the other the state of the blood.* Of course the case in which no disease of the heart exists will be the most favourable for treatment; and it is fortunate that in many cases of recognized aneurism of the aorta, the healthy or diseased state of the heart can be accurately determined during life.

In this case the pulsations of the vast tumor might well be

* See Pirry, *Journal Hebdomadaire*; Hope, *Diseases of the Heart*.

called torpido, and yet the heart was found small, flabby, and shrivelled. The explanation of the great force of aneurismal pulsations seems as yet far from being cleared up. Some have explained it by supposing a hypertrophy of the left ventricle: yet it may occur, as in this case, with the very opposite state of the heart. Others conceive that it arises from the co-existence of aortitis, an explanation which might apply to the case of true aneurism, but not to that by perforation, where the whole sac pulsates violently.

It appears to me that much of this violence of pulsation is explicable by the common law in hydrostatics, that a given pressure exercised on a fluid confined in a vessel is communicated to every portion of the surface of that fluid, and thence to the periphery of the vessel, and thus multiplied. In fact we may draw a close analogy between the hydraulic press of Bramah and the use of a heart acting upon an aneurismal tumour of the aorta, which contains a quantity of blood, and thus arrive at an explanation of its enormous pulsations; of course the effect will be diminished or modified by the quantity of coagula; the elasticity of the sac, and the means of exit of the fluid. I need hardly remind the reader, that it is during the diastole of the tumour that the violent pulsation is felt, a point strongly bearing on the explanation of the impulse of the heart itself.

I have now great pleasure in inserting the following communication, with which I have been kindly favoured by Professor Harrison, on the subject of Arterial Pathology. When we consider its source any observations on its importance would be superfluous:

“ January 6th, 1834, 1, Hanover-street.”

“ MY DEAR STOKES,

“ I hasten to reply to your note I have just received, in which you request me to convey to you in writing certain remarks on thoracic and abdominal aneurisms, the substance of which I lately communicated to you in rather a desultory conversation.

“ In alluding to the pain, among other symptoms attending

internal aneurisms, I have remarked, that although it is very frequently stated that the degree of suffering attending thoracic aneurism is trivial, when compared with that complained of in a similar lesion of the abdominal aorta, yet exceptions are not unfrequently occur. Thus, I have known cases of aneurism of the arch of the aorta to have been accompanied almost through the entire course with intense, though often intermitting pain in the region of the heart, in the spine, or through the chest generally, or in one or both arms; while, on the other hand, I have seen instances of this disease in which the suffering was so trivial, and the symptoms were so faintly and equivocally expressed, that the disease was not ascertained until the post mortem examination revealed its real nature. In forming our diagnosis, then, in suspected cases of thoracic or abdominal aneurism, too much importance or reliance is not to be placed on the mere circumstance of pain being either slight or absent, or intense and of almost continued duration. The change in a blood vessel denominated aneurism is of slow progress, and unattended by acute inflammation; the arterial tissue, though peculiarly organized, is not very sensible to pain, or subject to acute disease, therefore aneurism is not painful *per se*, but only in proportion as the tumour may interfere with some adjacent organ, or excite irritation in some contiguous sensitive structure. Thus aneurism in the arch of the aorta may disturb the action of the heart, or may irritate the trachea, or by extending, compressing or in any way disarranging the neighbouring nerves, such as the left recurrent, the phrenic, or the cardiac plexus, may give rise to severe paroxysms of pain, both local and remote; yet, in other instances of disease in nearly the same situation, the tumour may so shape its course as to steer clear of any interference with surrounding objects, and thus avoid all excitement or irritation, or even interruption, to their functions, except so far as the latter may be impaired by the abnormal condition of a vessel whose integrity must be so very essential to the existence and well being of the economy at large. The

same remark may apply to aneurism of the thoracic aorta in the posterior mediastinum; the tumour in some cases proceeds through all its stages to its fatal termination, without much accompanying local pain, or decided premonitory indications, whereas in other cases, acute spinal irritation, difficult and painful respiration and deglutition, are almost constant concomitants: attention to the anatomical connexions of the vessel in this situation, and to the different directions it may take in different instances, may explain the variety of symptoms that present in different cases.

“ The abdominal aorta is in connexion with several important organs through its whole course ; it is also invested in *three-fourths* of its circumference with a plexus of nerves, branches of which extend along the different arteries to the several viscera. All the visceral arteries are remarkable in this respect, but more particularly the renal, gastric, and hepatic ; one continued nervous network surrounds each from its origin to its final distribution. From these circumstances, we should expect that aneurism of the abdominal aorta, or of its branches, should be attended with much pain and functional derangement ; and such is generally, but not uniformly, the case. I have witnessed the dissection of three cases of large aneurism which had burst behind the peritoneum, and which, during life, had not been attended with much pain, or with any decided symptoms which could lead to a positive diagnosis. In each of these the tumour was connected with the posterior part of the artery ; in one case it was situated close to the diaphragm, and the blood was partly injected into the posterior mediastinum, and partly into the abdomen, about the pancreas ; in the second case, the tumour had burst into the cellular tissue round the kidney ; and in the third case, the symptoms had been during life so similar to those of psoas abscess, that uncertainty as to the true nature of the disease occasionally existed. (See *Surg. Anat. of the Arteries*, vol. ii. p. 25.) Although I have not sufficient facts before my mind on which to ground the assertion, I should yet consider

that when aneurism arises from the *forepart* of the abdominal aorta and extends into the cavity, pain, and more or less disturbance of the adjacent viscera, will be more likely to occur. The stomach I have remarked to sympathize extremely in these affections; hæmatemesis has frequently occurred during their progress, and in one case death immediately followed a copious vomiting of blood, which must have been supplied from the capillaries of this viscus, as the aneurismal sac had no communication with it, and had poured its contents in a totally opposite direction.

“ In speaking of abdominal aneurism, I am led to another remark which appears to me to present some interest, namely, that in the abdomen we frequently meet with aneurisms of the *smaller* arteries, not merely the coeliac axis, or the mesenteric, but of the gastric, splenic, hepatic, &c. I have seen examples of each of these. I also recollect a remarkable case, in which, on opening the abdomen, I was surprised at the livid colour and great size of the omentum; it was injected with grumous blood, from the arch of the stomach to near the lower border of this process. On careful examination I found it had proceeded from an aneurism of the left gastro-epiploic artery; the tumour was about the size of an egg, and had given way by a sort of slough between the layers of the gastro-colic omentum; the surrounding parts were slightly thickened, but the stomach was free from disease, as was the peritoneum from inflammation. In the Museum of the College of Surgeons there is a specimen of aneurism of the coronary artery of the stomach. I have seen one case of aneurism of the right hepatic artery, including the root of the cystic; the tumour had not opened; the patient died of dropsy and disease of the heart; the gall bladder was shrivelled and empty. I have dissected a case of large splenic aneurism which caused death by bursting behind the peritoneum: the spleen was unusually small. Many years ago I saw an aneurism of the right spermatic artery about an inch distant from the aorta; the testicle was of the usual size, and appa-

rently healthy ; the tumour was about the size of a nut, and had not caused any injury to the surrounding parts, though it is probable, had the individual lived for some time, it would have proved fatal by bursting. In the course of last winter, I accidentally found in the dissecting room an aneurismal tumour on the left renal capsular artery ; the subject was a female, about eight or ten years of age ; the tumour was solid to the feel, and on cutting into it was found filled with successive laminæ of fibrinous matter ; very little cavity remained, so that the disease might be considered as undergoing a natural or spontaneous cure. Although aneurism is a disease common to all parts of the vascular system, yet it is certainly very seldom found in any of the external arteries, except the principal trunks, such as the carotid, axillary, iliac, femoral, or popliteal. I do not recollect any case of spontaneous aneurism in any of their smaller branches, though such are not unfrequent consequences of wounds or injuries ; even in an artery of the size of the brachial, spontaneous aneurism is very rare. In the cranium it has been met within the internal carotid, and in some of its primary branches ; it has been also found, but very rarely, in the coronary arteries of the heart. It might prove an interesting inquiry to consider why the small arteries of the abdomen more frequently present this disease than arteries of a similar size do in any other situation. Is the cause of this fact to be found in any peculiarity of structure, of course, of connexion, or in function ? probably on each of these circumstances it more or less depends. As to structure, the abdominal arteries are certainly very weak, particularly their middle coat ; hence their great liability to rupture in the common process of injecting the dead body. As to course and connexion, no vessels in the body are more remarkable for numerous turns, angles, flexuosities, &c., and most of them run very much unsupported, except by the general pressure of the parietes of the abdomen. In function, too, many peculiarities present themselves to our attention which may serve to explain the frequent occurrence of this disease in the abdomen, as :

well as the great liability of the mucous surface of the alimentary canal to severe and frequent hæmorrhage: thus, the quantity of the blood circulating through this cavity must be very considerable; the active function that is so frequently exercised in *different* parts of the apparatus must require a proportional supply of blood, so that increased vascular actions must constantly occur in different situations and in rapid succession. The very remarkable freedom of anastomosis between the several vessels may probably be designed to facilitate the more rapid course of the blood from one organ to another, according as the respective functions of either may require it. The irritability of the small vessels may be inferred from the great supply of nerves with which they are furnished. I have not sufficient facts before me to decide on the pathology of these small abdominal aneurisms. I have not observed any diseased condition of the coats of the vessel in the vicinity of the tumour, so commonly observed in thoracic aneurisms. From the observations I have made, which, however, are insufficient, I consider that they commence by rupture, and not by dilatation of the internal tunics.

“Yours very truly,

“ROBERT HARRISON.”

Among the many interesting and important subjects alluded to in this letter, there is none more interesting than the explanation of the comparative frequency of aneurisms of the internal, as compared with those of the external arteries of the body. I have long thought that this was to be explained by a reference to the sympathies of arteries with organs.

If we look to the viscera we see numerous parts and separate organs constantly performing various functions, and liable to separate physiological and pathological excitements, while the system of voluntary muscles, in the first place, represents a single organ, is often at rest, and is composed of a tissue but little liable to inflammatory disease. Hence, in part at least,

the comparative rarity of disease in its minor arteries, alluded to by Professor Harrison.

This leads me to notice a species of arterial throbbing either in the abdominal aorta itself, or, as has been suggested by my friend, Dr. Houston, in the second order of vessels, which has not yet been described.

As a sign of diseased action, throbbing of the abdominal aorta has been long recognized. But its nature in all cases does not seem to have been discovered. It has been described as a nervous phenomenon; as the result of the pressure of tumours on the vessel, and as a symptom of aortitis. It may also be seen in cases of great retraction of the abdomen. But there is a pulsation of the abdominal aorta or its immediate vessels, which is symptomatic of inflammatory disease in the digestive system, and which a long experience enables me to say may be considered an important assistance in diagnosis. A throbbing generally commensurate with the disease; removed by treatment calculated to relieve enteric inflammation, and aggravated by every thing which will increase this affection. In other words, we may have from enteritis or peritonitis a throbbing of the abdominal aorta or its vessels, perfectly analogous to the morbid action of the radial artery in whitlow, or of the carotids or temporal arteries in cerebritis.

The cases in which I have most frequently observed this symptom, are those of the gastro-enteric fever of this country; and when we reflect on the latency of the follicular ulcerations of the intestine, and the great number of times that this lesion is overlooked, and exasperated by improper treatment, we must see of what importance the knowledge of any prominent symptom must be. I do not say that it occurs in all cases, such is far from the fact; but I know that it occurs in many, and may be made a great assistance in diagnosis.

I have also found it in cases of fever after corrosive poisoning, where the pulse was almost absent at the wrist; and in peritonitis, where no pulse could be felt.

In these cases we have frequently the following group of circumstances: fever, prostration, thirst, tenderness of the epigastrium or the ileo-cæcal region. The pulse at the wrist is often small and feeble, while the abdominal pulsations are comparatively violent. In most cases the other symptoms of gastro-intestinal disease are sufficiently plain. But in several instances this want of proportion between the action of the radial, and abdominal arteries, combined with fever, has been the principal indication of enteric disease.

As might be expected, the increased action may extend to the femoral arteries. This fact enabled me on one occasion to arrive at an accurate diagnosis. A patient who had been subject to chronic disease of the stomach, was brought into hospital in a dying state. Something, he said, suddenly gave way within him a short time before admission. His countenance was collapsed; the hands blue, and no pulse could be felt at the wrist. The belly was swelled, but not very tender.

It was suggested that the case might be a rupture of an abdominal aneurism; others supported the opinion that it was an example of peritonitis by perforation: and that the latter diagnosis was the right one, I concluded from the fact, that while no pulse could be felt at the wrist, the femoral arteries at the groin were pulsating strongly. Recollecting also that the patient had laboured under symptoms of chronic gastritis, and that the feeling of giving way was felt first in the left hypochondrium, I concluded that the case was one of peritonitis from a perforating ulcer of the stomach. On dissection an old circumscribed ulcer of more than an inch in diameter, was found in the greater curvature of the stomach. This had perforated the peritoneal coat by an irregular rent. The aorta was healthy.

In some cases we have found that the pulsation was strongest in the direction of the right iliac artery, and subsided with the symptoms of the gastro-enteric fever. We have seen it with and without the occurrence of diarrhoea, and frequently reappearing

with relapse, particularly after errors in diet, or hyperæthrisia. In some instances it has subsided when the diarrhoea appeared, and returned on its being arrested by an astringent. The knowledge of this sympathetic pulsation of the great vessels of the abdomen may throw light on the supposed cures of internal aneurism, by the system of Valsalva. It must be recollected that the cause of this pulsation (which is often violent) is most frequently a local inflammation of some part of the digestive system; in other words, an affection likely to be relieved by such a mode of proceeding. Indeed the suggestion of Berlin, that some of these cases were not really examples of aneurism, seems at least so far, highly probable. Experience seems to show that in many cases a low diet aggravates the symptoms of large internal aneurisms; and if the explanation which I have given of their violent pulsations be correct, we can easily see that, the more fluid be the contents of the mesenteric cavity, the greater will be the impulse from the hydrostatic pressure; of course this would act in extending the sac, and, consequently, injuring the surrounding parts. In the case of aneurisms, the subsidence of the impulse is one of the first and most important signs.

ART. XXV.—Description of the Calcule Fractor for Pulverizing the Calculus in the Bladder. By FRANCIS L'ESTRANGE, A. M., M. R. C. S. I., Dawson-street.

FROM the earliest ages it appears to have been a desideratum in surgery to discover some means whereby the dangerous operation of lithotomy might be rendered unnecessary; thus we find various preparations at different times tried and extolled for their supposed solvent powers. It does not seem, however, that the grand object in view was destined to be attained until modern invention should contrive the expedient of breaking up the calculus in the bladder, and having the detritus subsequent-

ly washed out through the urethra. It would be quite superfluous to draw a comparison here between lithotomy and lithotripsy, that has already been ably done by the different gentlemen who have written on the former operation. Still it must be admitted, that however good the principle, yet in practice this operation has been found to labour under many defects. For example, with Baron Heister's *Retractor*, to which, taken altogether, appears to be the most perfect of the instruments as yet used for this purpose, there is almost a certainty of the instrument, or alone held by it, or both, being driven by the hammering against the sides of the bladder, and thus causing incalculable mischief. So great a defect has this been found, that to obviate it a bed has been constructed, and the patient being fixed thereon, the *peruteur* is introduced and fastened to an iron bar placed in front for that purpose, the staff is thus certainly rendered almost immovable, but as the patient cannot be equally well secured, and as the slightest motion on his part, might cause as great or even greater mischief, the object seems to be as far as ever from being accomplished. Nothing is here advanced against the inconvenient unwieldiness and great expense of such a complicated system of instruments, beds, &c. Again, the detritus of the calculus, by lodging between the curved blades of the instrument prevents their closing, and thus increases, in some instances, considerably, its diameter; this, in all cases, must cause extreme pain on the withdrawal of the instrument, and has in particular instances produced laceration of the parts and hæmorrhage.

To do away with these and other objections, the instrument, of which sketches and a description accompany these remarks, has been invented. Its advantages are *extreme simplicity*; it being acted on by a screw of such power as to pulve-

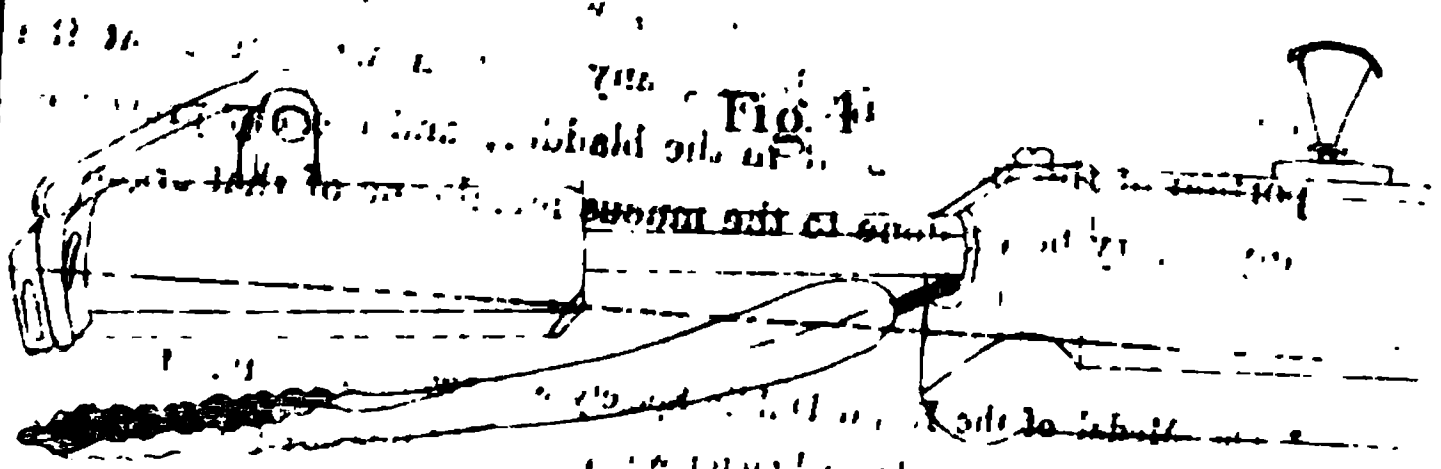
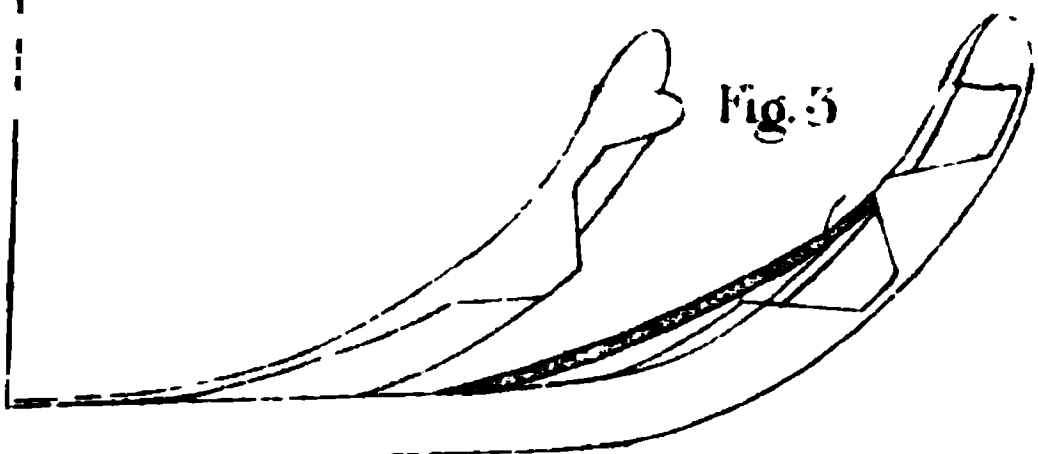
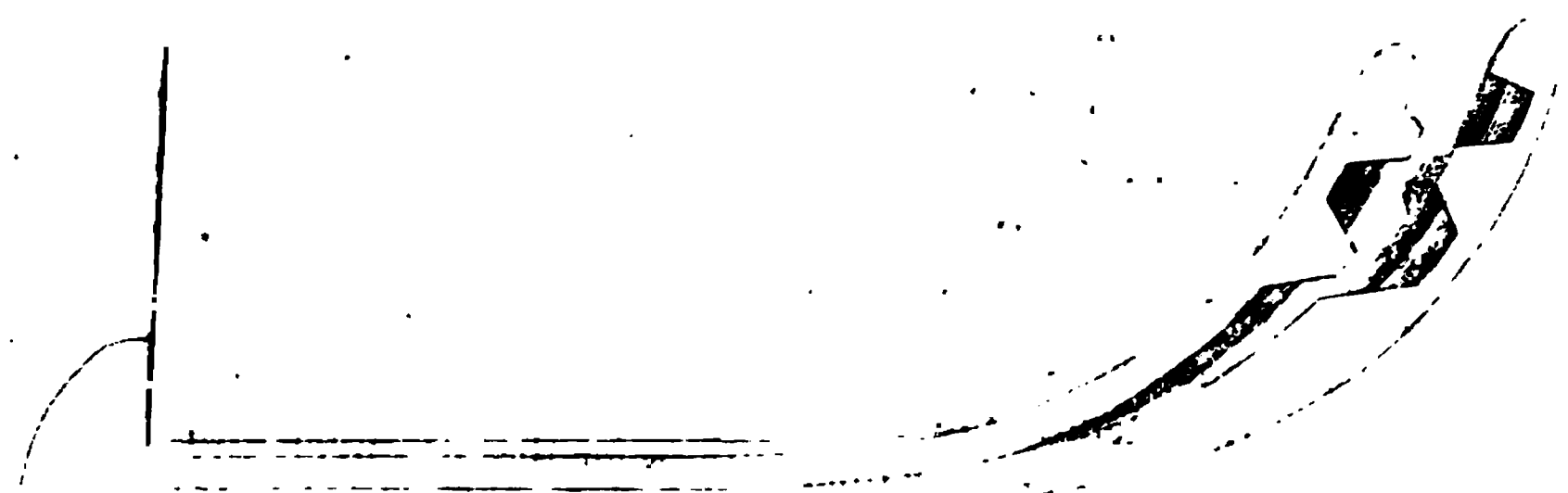
however, it does not seem to be a new invention, but a modern improvement on the old one. It is a screw of such power as to pulve-
 • Vide *Lithotomy*, *Lancet* 1829, 30, 31; *Lithotomy and Lithotomy compared*. T. King, M.D. 1832.

rise any calculus found within the bladder, thus doing away with the dangers of percussion; a stilette, which passes in a groove between its blades, by which the detritus lodged there can be at once removed; a claw fastened on a pivot to the screw, so as to allow of its being fixed at will to the upper blade of the instrument, which can then, by turning the screw backwards, be separated *without the slightest shock* from the inferior one, should these have become agglutinated by the broken down calculus and its animal mucus. Finally, should any case occur, or any individual still prefer the operation with the hammer, by removing the vice this can be performed as heretofore, the operator enjoying the additional advantage of the stilette. There are many other desirable qualities that might be enumerated as possessed by this instrument; but it is not intended to trespass at present any further on the reader's time; suffice it to say in conclusion, that there are but two cases requiring the operation of lithotomy to which it is not applicable, namely, a calculus contained in a pouch, or a bullet in the bladder.

EXPLANATION OF THE ANNEXED PLATE.*

FIG. 1. Represents the instrument as in the act of being closed by the power of the vice, the point of the screw playing in a socket at the extremity of the handle, which causes direct pressure downwards, without producing any rotation whatsoever at that portion of the instrument in the bladder, and thereby preventing any injury being done to the mucus membrane of that organ.

* The Medal of the Royal Dublin Society was awarded for this Instrument. The operation with the Calculo Fractor was performed this day (June 4th), with complete success, by the Surgeon General, (there were present also Surgeons Adams, Hargrave, and Smyly,) the peculiar advantages of this Instrument having been exemplified in a remarkable manner.



The present invention relates to a new and improved
 method of manufacturing a shoe sole, and more particularly
 to a method of manufacturing a shoe sole which is
 adapted to be used in a shoe of any style or make.
 The method of the present invention is characterized
 by the fact that it provides a shoe sole which is
 adapted to be used in a shoe of any style or make.
 The method of the present invention is characterized
 by the fact that it provides a shoe sole which is
 adapted to be used in a shoe of any style or make.

The first part of the instrument is a double thread. The second part of the instrument is an advantage to the operator by enabling him the better to keep steady the instrument, this part is held in his left hand which is to be supported on his left knee, the foot being placed on a low stool in the position for operating.

Fig. 2. The claws of the instrument are kept open their wedge-like ends are adapted for cutting also the instrument is adapted for cutting the claws so as to remove all parts of the claws which always remain present. The instrument is adapted for cutting the claws of the bird and is adapted for cutting the claws of the bird.

Fig. 3. The box of the instrument is adapted for holding the instrument in the position of the instrument when the instrument is placed in the position of the instrument.

FIG. 2. The vice, having a screw, with a double thread. The weight of this portion of the instrument is an advantage to the operator, by enabling him the better to keep steady the instrument, while this part is grasped in his left hand, which is to be supported on his left knee, the foot being placed on a low stool in the position for operating.

FIG. 3. The claws of the instrument open their wedge-like teeth well adapted for splitting, also the detritus rod scraping the bottom groove, so as to remove all particles of the broken stone that always remain preventing the claw from being closed, and thereby causing great laceration of the urethra and hæmorrhage.

FIG. 4. The box of the instrument, the handle of the upper claw in which the socket for the screw to play in is placed, also the canal for the detritus rod.

BIBLIOGRAPHIC NOTICES.

An Introduction to Botany. By JOHN LINDLEY, F.R.S., L.S., G.S., Member of the Imperial Academy Naturæ Curiosorum, Professor of Botany in the University of London. With Six Plates and numerous Wood Engravings. London: Longman, &c. 1832. pp. 557, 8vo.

EVERY one knows that there are two qualifications in a scientific work which should influence both the author in undertaking it, and the public in the reception of it, first, that it should be a desideratum, and secondly, that the desideratum should be well supplied; if deficient in either of these it cannot expect the privileges attending a literary work, of making up for defect in matter by excellence of style, but must remain an useless and unnecessary addition to the heap that obstructs the student's path towards the attainment of knowledge, which should be acquired with the least possible waste of time and words. Under both these points of view we shall consider Professor Lindley's work, as they are equally applicable to botany as to all the physical sciences, so long as any investigations remain to be made, and consequently any thing new remains to be taught.

The greater part of our knowledge, both of the natural affinities of plants and their organic structure, has been furnished not by the general botanist, properly so called, but by those who have devoted their attention to the habits and characters of a certain number of tribes, and to the examination of one or more organs in all their various modifications or changes of aspect; and the works which treat of a single natural family with all its species, now generally called monographs, have made us perhaps better acquainted with plants than the invention of any artificial system, except the Linnæan alone. The accumulation of facts thus increasing by the daily investigations of philosophers, each in their own particular subjects of research, are for the most part scattered through philosophical journals or foreign publications, not always easy to be procured, and often unintelli-

gible to the general reader from the abstruse and complex manner in which they are written. To collect these facts together, to present them in one unbroken tableau, to arrange them synthetically so as to throw light on the whole system, is the office of an introduction to botany, and its author is seldom more than a redacteur of the minute and laborious investigations of others, as many a tolerable botanist has never read the original works of Bonnet, Amici, or Dutrochet, and many of the same class would have never heard of these philosophers or their discoveries but for the same easy and convenient medium.

Introductions to botany, therefore, besides their value in initiating the learner into all the elementary knowledge requisite to be possessed before pursuing his own observations, become a kind of evidence of the progress of the science, a statement of the increasing quantity of facts which have been discovered in the different stages of its improvement, and resemble, in some sort, resting-places on the ascent of a mountain from which the prospect is seen to grow more extensive the higher we proceed. If we compare the early elementary works containing a few simple, *prima facie* characters, perceptible to the commonest observer, with the *Philosophia Botanica*, that has laid down the rules and furnished the matter for so many succeeding works, (in most of which we perceive a gradual disposition to depart from the simplicity of their model,) and this last with the abstrusely analogical treatises of the modern French school, we shall find that the fullest elementary works written a few years back will leave us totally ignorant of some of the most important truths respecting vegetable organization and natural relations, a perfect knowledge of which seems the *ne plus ultra* of botanical philosophy. It is evident, therefore, from the mass of facts which modern discoveries are daily pouring in, that an introduction is absolutely necessary at least every three or four years, and one published at any moderate space of time after another can scarcely fail to contain a considerable quantity of information which was wanting in the antecedent.

The best works (at least those with which we are most familiar) are Richard's and De Candolle's Elements, both published within a few years, and containing the new philosophical theories which originated principally among the French. The latter work we believe was the first that gave a popular exposition of the cohesion and adhesion of vegetable organs by which so many of the anomalies in natural affinities are explained, and the former, as translated by Dr. Griston in 1820, was the text book for Irish students during the following years; but new discoveries, particularly in microscopical botany, have added so much to the bulk of acquired knowledge, and thrown so much light on

some uncertain, though not unimportant points in physiology, that a new elementary work was partly again to be desired, and the desideratum Professor Lindley has, if not fully, at least sufficiently well supplied. But we must not suppose that his introduction from its additional stock of information should altogether supersede that of Richard, which is superior to it at least in one respect, from its giving a detailed account of a branch of the subject, which the former work has left altogether untouched, as we shall show hereafter.

Professor Lindley has arranged his work in seven books, of which the first and longest is occupied with the structure of vegetable organs in all their modifications, according to the latest theories of organography afforded by Mirbel, De Candolle, and others. The most interesting part of this book is the opening chapter, in which he gives an explanation of the elementary organs of vegetation, commonly called vegetable tissues, so necessary to be known when we consider the importance of anatomical peculiarities in determining the primary divisions of the vegetable kingdom, and how imperfectly it has been hitherto explained or understood. Except in determining the nature of cuticular stomata and the termination of spiral vessels, Professor Lindley has not been able in this book to give us much original information of his own, but he can adduce his own evidence to many facts which bear the authority of others, and it is often not altogether unsatisfactory that a second person can vouch for the reality of anatomists' discoveries. This chapter contains a minute and detailed account of all the facts that late investigations have proved or attempted to prove respecting cellular and vascular tissue, and in this particular it is superior to any elementary work on general botany, which has yet been published. But the student must be prepared, in commencing this chapter, to abandon many opinions which he had formerly received as axioms, such as the nature of fibrous tissue, and the existence of various species of vessels distinct from the spiral. His account of the microscopic structure of woody fibre and spiral vessels is particularly worthy of attentive perusal. In the next chapter he proceeds to the consideration of compound organs, the rough knowledge of which is much more necessary to be acquired than of the elementary, as the arrangement of every system is based more or less on their form and characters, and as the nature of tissue, from its minuteness, requires the aid of good glasses and too nice a process of examination to be easily studied by learners. It is very desirable that both the external appearance and anatomical structure of these compound organs should be correctly and intimately known, as it is by their various modifications that all the laws of vegetable life are carried on, but

it appears to us that Professor Lindley has spun out to too great a length his description of every minute portion that enters into their construction, and perplexes with too many minutiae the botanical student, who, in an introductory work, seeks rather for a clear and explanatory statement of whatever facts are really important, than a profound investigation into all the various opinions respecting their nature, which different writers have promulgated, and the various names by which, at different times, they have been designated. It is too much the custom among botanists, when an uncertainty exists relative to the function of any particular organ, or to the class or species to which it is to be referred, to remedy or attempt to remedy the doubt by suggesting a new theory, or publishing a new opinion of their own, equally uncertain as to accuracy, or rather equally remote from the truth, a kind of homœopathy by no means agreeable to the student, to whom the mass of conflicting doctrines among which he has to select a belief, is a remedy worse than the evil. It would be desirable that Professor Lindley had not overloaded his text with such a profusion of synonyms, they would better have been collocated in a glossary at the end of the work. As a specimen of his redundancy on this subject we shall quote the following passages :

“*Stomata*.—Pores of the epidermis; pores corticæ, allongés, évaporatoires, ou grands pores; glands corticales, miliaires, or épidermoïdales; glandulæ cutanææ. Oeffnungen; *Stomatia*.”—p. 33.

“*Netted leaf*.—It is the *folium venosum* of Lin.; the *f. imbricatè venosum* of Link; the *f. mixtinervium* of A. Richard; and the *f. retinervium* of De Candolle. If the *venæ externæ* and *venulæ marginales* are conspicuous, Link calls this form *combinatè venosum*; but if they are indistinct he calls it *evanescentè venosum*.”—p. 92.

The second book treats of physiology in all its branches, and we wish our limits would permit us to give a long extract from the first chapter, which is an exceedingly good epitome of the manner in which the two chief modifications of vegetable structure, and consequently vegetable life are developed, and the laws by which their vital functions are regulated; but we can only recommend it to the attentive perusal of all who wish to be acquainted with that very interesting subject. In the following chapters he proceeds to the examination of the physiology of these organs taken separately, after having considered their combined action in the entire vegetable. The whole book is by far the best arranged of any in the introduction, and contains the greatest quantity of valuable information, given in the clearest and most pleasing manner. In the third book, entitled *Taxonomy*, he proceeds to explain the principles of classification, and the two great

systems to which these laws are applied. The first chapter of this book is devoted to the consideration of artificial classification, and here we must say a few words, in passing, to deprecate the spirit of invective with which many disciples of the modern school of botany allow themselves to speak of the Linnæan system, and its ever illustrious author. Between the systems of Jussieu and Linnæus there can be no rivalry, for they can or ought to exist independently of each other: to the first may be allowed its profounder philosophical views, its more extensive knowledge of nature, and the superiority of having grouped the vegetable kingdom as near as possible to the manner in which they were primitively arranged by the Creator himself; but still for ascertaining a new plant we must acknowledge the absolute necessity of the Linnæan system, or some other artificial arrangement, no matter how arbitrary are its rules, or how far it departs from natural affinity in its grouping so as it presents a few obvious characters for distinguishing genera and species; such a method must exist as long as there are new plants to be discovered, or new botanists to find those plants which are already described. Between the two no comparison can be fairly drawn; they differ in their characters and principles as they do in their use, and when we hear of the imperfection of the Linnæan system, as relative to the natural method, we must beg that another system will be invented by which a new plant can be ascertained at first sight before it can be studied in its more important station among the natural families. The third chapter commences with an explanation of the principles of natural classification, which are given in a few words with more conciseness and perspicuity than we have seen any where. He says—

“A natural method of arrangement differs essentially from an artificial one in this, that it does not depend upon modification of any one part more than of another. Its divisions are framed from a careful consideration of every, even the minutest, character that is appreciable, and consist of species, not arbitrarily collected by a few common signs, but agreeing with each other as far as possible in every material point of structure. Groups formed upon this principle will necessarily consist of species having a greater resemblance to each other than to any thing else, and if skilfully constructed, will have so great a general resemblance, that a knowledge of the structure, habits, qualities, or other important peculiarities of a single species, gives an accurate general idea of all the others that the group contains.”—p. 318.

But we think Professor Lindley has erred in giving us the original form in which Jussieu published his system, instead of

the more modern arrangements, which, being the latest, must be supposed the best. Richard, in his *Elements*, has here given us a separate account of each of the natural families, which, together with the illustrating notes of the translator, makes us in some degree familiar with the whole system. This omission is a great want in Lindley's work, and might be supplied to the exclusion of less useful matter. The fourth book is occupied with glossology, separated with great judgment from organography, with which it is too often incorporated. Every student should make himself pretty well acquainted with those botanical terms to enable him to discover and describe plants with facility. The next book treats of phytography, or "The Rules to be observed in describing and naming Plants." We are of opinion that much superfluous matter might be omitted here, and all the information that is of importance added to the chapter on the value of characters in the third book. The geography of plants occupies the sixth division, a subject of great interest, and very deserving of being accurately investigated. All the observations that have as yet been made on the subject have furnished us with few certain data, as so many unknown causes, independent of latitude, or any apparent differences in soil or other local circumstances, influence the appearance and disappearance of plants. Professor Lindley has in this book given a compendium of all the principal discoveries which Humboldt and others have made on the geographical distribution of plants, and every person who has it in his power should endeavour to add some knowledge to the little we possess on this very interesting branch of botanical philosophy.

In the seventh and last book we finally come to the consideration of the theory of morphology, which we shall explain in his own words:—

"That part of botany which treats of the gradual transmutation of leaves into the various organs of a plant, which shews that bractæ are leaves affected by the vicinity of the fructification, that the calyx and corolla are formed by the adhesion and verticillation of leaves, that the filament is a form of petiole, and the anther of lamina; and, finally, that the ovarium itself is a convolute leaf, with its costa elongated into a style, and the extremity of its vascular system, denuded under the form of stigma, is called morphology."—p. 504.

We have heard that some heathen philosophers defined the soul as harmony, others as numbers, &c.; but here we find that the life, soul, and essence of vegetation consist in a leaf. This evidently is considered a *bonne bouche*, and as such reserved for the last. In the book on organography the subject is delicately hinted at, and brought in agreeably with the principles of or-

ganization, from which it emerges like a phantasm from the windings of a magician's wand. The doctrine is certainly true in many respects, and, according to the prevailing taste for analogy of every kind, appears extremely philosophical; but on a little examination, we shall find that the theory is neither so very wonderful, so very correct, or even so paradoxical, as it appears at a first glance. When we consider the extreme simplicity of the chemical basis of vegetable matter, and the equally simple nature of the tissue which is formed by the combination of those elements, we can easily conceive that the organs of vegetables must partake more or less of the same nature, and that the character of one organ will merge into that of another, if influenced by causes which are for the most part hidden from our perception. Nature seems to have imposed one inviolable commandment on vegetation, that of the ascending and descending caudex of terrestrial plants, a problem which most likely never will be solved but by considering it as a fiat of the Creator. The other principles of growth may be considered laws of discipline, which may be altered by some internal circumstances of its development with which we are unacquainted; this being the case, we should not wonder if the cellular and vascular tissue, which is usually employed for the elaboration of some peculiar nutriment, might, by the addition of much aqueous fluid, (Dutrochet would say a superabundant endosmose), lose their power of forming those refined secretions, and spread into a less highly organized formation, as stamina into petals, sepals into leaves, &c. Of regular metamorphosis, such as the constant and uniform change of whorls of leaves into calyx, corolla, stamina, and ovaria, of which the seeds are modified buds, it is perhaps more difficult to prove either its accuracy or its utility. The only remaining doubt seems to be, whether these organs actually were the rudiments of leaves in the early period of their formation. The belief that they were seems now generally received as an explanation for all difficulties, for we hear that the writings of Linnæus and Gærtner are "a mere tissue of ingenious misconceptions" (speaking of the pistillum). De Candolle, Du Petit Thouars, and Göthe, have proved that it is nothing but a leaf! The plan upon which all vegetable beings are created is so uniform, and the rules under which their structure is modified are so simple and economical, that we might be tempted to refer this apparent change rather to analogy or similarity than to identity of organization. The pericarpium is formed of veins and cellular tissue corresponding to the parenchymatous and vascular lamina of leaves, the ovula resemble buds, with this difference, that the latter are not furnished with

an ascending and descending tendency. But must it follow from this, that they actually are modifications of leaves? For what purpose should nature create an organ with the design of subsequently changing it into another? On this subject much may be stated for and against, nor are we prepared to give our opinion arbitrarily on it; but certainly the explanation of interest has not the same talismanic effect on us of clearing away every difficulty in the nature of vegetable organs, as some have imagined, nor does the term carpellary leaf suggest a clearer idea of its structure than a many-called ovarium. At all events, whether their nature is or is not changed, they are at the period of their development a distinct organ, and in botanical terminology they should be named as such; for as we know that the foot, from similarity of structure, may execute the functions of the hand, (different from any other organs of the human frame, which are formed each for their own purposes, and incapable of being used for another), should we for this reason apply the same name to designate both? It will probably soon become highly unphilosophical to call the human arm by any other name than that of superior leg, and the former word will cease to be used, except by dress-makers and tailors, to distinguish the members that are suited with a pair of sleeves from those which are usually accommodated with a pair of trousers. But to speak seriously, the whole of this seventh book is exceedingly curious, and well worth the trouble of reading. The theory of morphology is so generally adopted, that every young botanist should make himself somewhat acquainted with its principles, and we leave it to his own judgment whether he will give it implicit credence or not.

We shall now conclude this article with declaring our favourable opinion of Professor Lindley's work; and, though we expressed our disapprobation of some points, they are far outnumbered by those with which we were highly gratified.

An Investigation into the Remarkable Medicinal Effects resulting from the External Application of Veratrum. By ALEXANDER TURNBULL, M.D. London. pp. 96.

This system of rubbing in the hands of St. John Long appeared of so much advantage, (to himself at least,) that a host of imitators instantly arose, all anxious to try this apparently wonder-working mode of cure. Every variety of local appli-

cation, which the utmost ingenuity could devise, was put in active requisition. Long forgotten medicines, which had had their day, were dragged from their obscurity and applied to this purpose, and several new ones, adapted for it, discovered. No doubt, in many instances, much good was done; but still the most uncandid were forced to admit occasional failures, and an ointment which should never fail was still a desideratum. This, happily, Dr. Turnbull has achieved, (if we are to believe the work before us,) by the discovery of the virtues of the *Veratria*, which his cases plainly declare to be infallible. This substance, he tells us, is the active principle of some of the family of plants, called *colchicaceæ*, and has the various names of *veratrinum*, *veratrina*, *veratria*, and *sabadilline*. It has been obtained from the *veratrum sabadilla*, *veratrum album*, and *colchicum autumnale*, and is not, as previously supposed, a simple body, but consists, according to Mr. Conerbe, of three distinct principles, *veratrine*, *sabadilline*, and *mono-hydrate of sabadilline*.

“The *veratria* of commerce is nearly white in colour, and in the form of a fine powder; it is without smell, but when accidentally or otherwise brought in contact with the mucous membrane of the nose, it induces violent and even dangerous sneezing; when applied to the conjunctiva it produces great irritation, accompanied by an abundant flow of tears, which does not subside for some hours. Its taste is extremely acrid, but destitute of bitterness; and it acts very strongly on the mucous membrane of the stomach and intestines: if introduced into the stomach, it proves highly emetic and purgative; for, even in old subjects, a quarter of a grain acts powerfully on the bowels; and in some experiments, the effects have been so violent as to show that death would have followed the administration of a few grains.”

Andral, jun. found, that when this substance was applied directly to the tissues violent inflammation was excited, and when thrown in small quantity into the intestine, it acted on the large intestine; and when in larger quantity, tetanus followed.

“*Veratria* has been prescribed both internally and externally. For internal use, it is given in the form of pill, or in solution, in dropsy, gout, rheumatism, &c.; and externally, it is applied either in the form of solution, or what is better, in that of an ointment made with hog's lard.” [Twenty grains of the *veratria* to an ounce of lard.] “And of this, a piece the size of a large nut may be rubbed from five to fifteen minutes, night and morning, as nearly as possible over the seat of the disease, until relief from the urgent symptoms be experienced; care being taken that the skin over which the ointment is rubbed be free from injury, otherwise considerable irrita-

tion of the part will ensue; and for reasons already given, the person who applies it must be careful not to insert even the smallest quantity under the eyelids."

Strange to say, its effects as an ointment, are almost the reverse of its internal exhibition, and far from irritating, purging, vomiting, &c., its effects are more like those related in an eastern fiction, or of Dr. Solomon's Balm of Gilead.

"It has been observed to calm irritation, remove pain, and produce considerable elevation of spirits. The general health and appearance improve; the appetite remains unimpaired, or even becomes better; the patient experiences not the slightest degree of nausea," and the bowels become so costive that purgatives are necessary.

The following looks so like magic, that for the sake of those who delight in the marvellous, we cannot resist quoting it:

"When the veratria is applied externally in dropsical diseases, the emetic and cathartic effects which result from its internal employment in these affections are exchanged for a diuretic operation, so singular and beneficial as to bring about the removal of the effused fluid, in a time much *shorter* than it can be accomplished in by any *other known medicine*; and it has done so in many cases after every other means had been previously tried without avail; but in diseases unattended by aqueous effusion, no effect whatever on the kidneys has been observed."

The unpleasant irritation which follows friction with tartar-emetic ointment, croton oil, &c. is not observed.

"The skin over which the ointment has been rubbed, even after the friction has been continued for some length of time, shows no marks of irritation, and although in certain instances a slight blush sometimes pervades the surface, it nevertheless vanishes in an hour or two, and the skin returns to its natural colour. When the quantity of veratria rubbed in exceeds a few grains, the patients generally experience a considerable degree of warmth and tingling in the part; and until this takes place the peculiar effects of the medicine do not usually manifest themselves; the circumstance, therefore, is one worthy of attention, and may serve as a criterion, whereby to estimate the extent to which the friction may be carried without producing inconvenience, and also to judge of the degree of purity of the veratria employed; for when, as is sometimes found to be the case, the medicine is mixed with impurities, these sensations are not produced, and its action is by no means either so certain or powerful as when it is free from adulteration. This last observation appears ne-

necessary to prevent the practitioner from laying a charge of failure against it in cases where the impure veratria may have been made use of in compounding the prescription.*

"After the ointment has been applied a sufficient length of time to put the constitution completely under its influence, the feeling of heat and tingling extends itself from the place where the friction may have been made, over the whole surface of the body, and in some instances involuntary twitchings of the muscles of the mouth and eyelids are induced; but these symptoms disappear so soon as the rubbing has been discontinued for a day or two, and no disagreeable consequences follow to the patient. The sensibility of the parts over which the application has been made, is increased to such a degree as to render them peculiarly susceptible of the presence of certain stimuli, particularly electricity or galvanism. These agents have, in some instances, been applied along with the veratria ointment, but have given rise to sensations so acute as to render their further employment almost insupportable, and that without the slightest perceptible alteration of the surface. It does not appear altogether necessary that the friction should be made exactly over the seat of the disease, for two cases have lately come under observation, in which the individuals who rubbed on the ointment, imbibed as much by the hand as proved sufficient to cure them of painful affections in distant parts of the body, under which they had been labouring for a considerable length of time before."—p. 8.

Some trials were made with the ointment on a surface denuded of its cuticle, but the consequent irritation was too great to render a repetition advisable.

We have given the above rather copious extracts on the nature and mode of application of the veratria, thinking that many of our readers who have not the book at hand might be anxious to have as detailed an account as possible on these heads. We think it but fair to the doctor to quote the following passage, which with singular modesty (not always the worst policy) he prefaces his miraculous cures:

"In an inquiring age like the present, it behoves an individual, in laying before the profession any new plan of treatment, especially if that be applicable to diseases which have heretofore been considered either very obstinate or incurable in their nature, not to say more in its favour than the facts brought to light during its investigation warrant; for experience teaches us that many remedies, the prudent use of which might have rendered the most essential service to medical science, have suffered, often irremediably, in consequence of the rash

* Pure veratria is entirely soluble in alcohol, and burns without leaving any residuum.

and inconsiderate praises heaped upon them by their discoverers. On this account it is intended to do nothing more than merely to state the cases as they have occurred, in the hope that they may not be thrown aside as savouring too much of the marvellous, but that they may induce other labourers in the same field," &c.

Before proceeding to lay before the reader some of his cases (which we have read with great care), we may repeat the observation, that he is much too modest in the remarks just quoted. Out of the forty cases given there is not a single failure, they are all cures! And these not in trifling affections, but most of them in what have been hitherto considered by the profession as incurable, and where we are told every thing likely to effect a cure in the usual way had been resorted to in vain. Nay, so far does the Doctor's anxiety to show the superiority of his own miraculous unguent carry him, that he has in some cases kept the patients six weeks, two months, and upwards, under his care, giving them the most powerful drugs our Pharmacopœia affords, (till, in one case, such debility ensued as enforced their discontinuance;—in all, the disease remained unsubdued,) before resorting to the veratria ointment, which had the alleged effect of almost instantly removing the disease, a result we see no reason to doubt would as readily have happened at first, to the infinite advantage of the patient's constitution and pocket, though in this last respect, not so much to the Doctor's. Another feature in these cases is, that most of them are as formidable from their duration as their nature, of sixteen, twenty-two, thirty-six years' continuance; and it is quite delightful to read of one of these ancient plagues of years' standing being removed by one friction of a few minutes. The curative power of the veratria is shown to be by no means confined to one class of diseases, but embraces a most satisfactory variety, including almost all the most common, obstinate, or incurable diseases, with the exception of consumption, the cure of which has been monopolized by St. John Long, Doctors Scudamore and Ramadge.

This uniform success in such very different diseases by the same remedy of course renders a very accurate diagnosis of comparatively small value; it matters little to you whether your patient labours under incurable "disease of the heart," "tic douloureux," or any of the thousand Protean forms of "neuralgia," "rheumatism, acute and chronic," "palsy," "apoplexy," "ascites," "anasarca," "ovarian dropsy," "amaurosis," "pains in the great cavities," "syphilitic nodes and pains," (several examples of the cure of each of which are presented to us,) all you have to do is to ascertain that he is diseased, or

thinks he is, and if so, to apply friction with the veratria ointment, which these forty cases, all cures, show, is equally to be resorted to in organic as functional diseases. Neither need you trouble yourself with age, sex, or constitution, as they tell us that the "sanguine," "robust," "stout-made," and "ruddy complexion," were treated precisely the same, and quite as successfully, as the "delicate," the "hysterical," those "of a weak constitution," and "spare habit of body," and the cures include all ages, from the "delicate boy of twelve," to the "old gentleman about seventy."

There is one thing in the composition of Dr. Turnbull's cases which we particularly regret, as affording ample food for the suspicious mind, we allude to the total omission (with the exception of two unsatisfactory exceptions at pp. 77 and 84) of names, dates, and places, the two last are never mentioned, and in lieu of the first we are plentifully supplied with letters of the alphabet, from Mr. B. down to Mrs. W. There is also a very remarkable similarity in the frequent recurrence of some peculiar and not very common symptoms, "pain down the left arm," "loss of memory," &c. The first class of diseases cured by the veratria ointment are, heart complaints, either organic or functional; we say organic, for though Dr. Turnbull merely holds out the hope of alleviation, and professes to repudiate the idea of cure in such cases, yet as he gives one or two instances, we do not see why we should reject this desirable effect of the veratria, so manifestly displayed:

"The ointment best adapted for use in affections of this class consists of fifteen or twenty grains of veratria carefully mixed up with an ounce of lard, and of this a piece the size of a nut is rubbed over the region of the heart for five minutes, once a day. In all the cases which have come under observation, a few such applications have generally proved all that was requisite either to effect a complete cure, or at least, to cause a cessation of the symptoms for a considerable time; the friction may then be employed at longer intervals, and should any slight accession take place it may be removed by a repetition of the same means."

This is very satisfactory news to the invalid.

"It has happened that from one application the symptoms, and particularly the palpitation, have been increased to such a degree as to render it impossible to induce the patient to submit to a second."

After this has subsided,

"In a day or two every trace of disease has disappeared. The urine is observed to be copious."

The first case is that of a lady who was almost entirely confined to an apartment of an equable temperature for seven years; with cough, dyspnoea, purplish lips, palpitation, &c. along with a numbness and loss of the use of the left arm. She had been under almost every variety of treatment without benefit, and who the Doctor (for a reason, having so speedy a cure in his hands, we may be permitted to guess), tried with purgatives, tartar emetic, croton oil, friction, &c., for a considerable time with some relief, but such debility occurred, that he looked on the case as hopeless even to veratria; in this, however, he was quite wrong, as

"In about a fortnight from the first application of the veratria, she was able to leave her room, and walk up and down stairs with facility."

And shortly after, every bad symptom having disappeared, the lady, who had never left her room for seven years, walked four miles to see her Doctor. He acknowledges this to have been, under ordinary treatment, a hopeless case of organic disease.

CASE V.—"Mr. W., aged 58, has been affected for seventeen years with palpitation, which, during the last seven of that period, has been attended by great difficulty of breathing, occurring in paroxysms, coming on especially whilst taking exercise on foot. The dyspnoea at these times has been so severe, and has been accompanied by such severe pain across the chest, as to compel him to lay hold on the nearest object for support. The pain, during the fit, extends down the left arm, and is of such intensity, that, in the words of the patient, no language can describe it.

"For these and other symptoms he has, during the last ten years, applied to all sources for relief, and almost every possible remedy has been tried, without procuring for him any ease. At the time he came under treatment, he had, besides the symptoms mentioned, a peculiar purple blush upon his face, and more particularly over his nose and lips; his voice was weak and quivering. Upon applying the ear over the region of the heart, a tremulous, confused, irregular pulsation was heard: the pulse was irregular, and very intermittent: the left side of the chest appeared much larger than the right, and the ribs over the cardiac region seemed pushed out, as if to afford a larger space for the heart's action. There was considerable distention of the abdomen, along with a very evident degree of enlargement on the right side, immediately under the margins of the ribs; his bowels had been for some time in a very torpid state, and required rather strong medicine to operate upon them; the lower extremities were a little swollen, and the urine deficient in quantity. His sleep was unrefreshing, and often disturbed by fits of coughing, which generally ended in great exhaustion."

An ointment of the usual proportions was ordered to be rubbed, for five minutes night and morning over the region of the heart; and, in the course of three days, the feeling of pain and constriction across the chest had entirely disappeared. As the pain in the left arm still remained, he was ordered to use frictions with the ointment to it also, and almost immediate relief from that symptom followed the application. In six weeks he could walk about, and up and down stairs without inconvenience; and rubbing the ointment when any symptom reappears, he is enabled to pursue his avocations with ease and comfort.

The cases we have just quoted are cheering enough, but not a bit more so than those which immediately follow of neuralgia, which do not appear less amenable to this pain-dispersing specific. Let us hear Dr. Turnbull.

"In no class of diseases have the beneficial effects of the veratrid ointment been more strikingly manifested than in that which it is now proposed to bring under consideration; for in none are the symptoms productive of more distress to the patients, and by no other means can the same degree of relief be afforded in so short a period."

Scarcely an exception, we are informed, (and none is given) to a cura occurs, after, as usual, having resisted every treatment for years. In tic-douloureux especially the anguish of years is dispersed in a few minutes by one friction. If the disease is not confined to one particular spot, but spread over the ramifications of the nerve, he considers it easier to subdue, and a smaller quantity of the ointment necessary. Where the disease is seated in a single spot, particularly in the frontal nerve, a continuance of the frictions are necessary. In the first-mentioned cases twenty grains to the ounce must be rubbed over the whole seat of the pain for fifteen or twenty minutes, "or what is better, till the heat and tingling caused by the friction have been so great as to produce" an effect equal to that of the disease itself, and then the friction may be discontinued for a short time, to see if the pain is less than before; sometimes one friction suffices, if not, it must be continued. Where the pain is circumscribed, to produce a full effect, forty grains to the ounce have been used. A caution is given to avoid letting any get in contact with the conjunctiva.

We shall give a case of cure, premising that we cannot help reprobating in the strongest manner Dr. Turnbull's conduct in this case, and wonder how, having a powerful remedy, if not a certain cure, (as he sets forth), in his hands, he could have the

heart to keep his unfortunate patient under the use of medicines, exercising the most powerful effects on the constitution, for two whole months, till, to use his own words, "her health was now much weaker, but her sufferings continued unabated," and never once apply that remedy.

"A lady, 55 years of age, of a spare habit of body, has been affected for the last 36 years with ~~an~~ ^{an} ~~douloureux~~ in the cheek, and in the forehead above the eyebrow, on the left side. From the commencement of the disease, she has had a paroxysm generally once a week, and at no time does she remember the interval to have been greater than fourteen days. Her sufferings during this attack have been extreme, and have compelled her to confine herself to bed until its termination; and it has never lasted a shorter time than two days. In the intervals, however, she has been perfectly free from pain, and her general health has all along been tolerably good. It appeared from the history of the case given by the patient, that throughout the long course of the disease, almost every kind of medical treatment had been put in practice without giving rise to any permanently good result, and in consequence of this circumstance, and as the symptoms appeared to admit of nothing but slight alleviation, if even that could be procured, she was ordered to keep the bowels open by the use of an aperient pill, and at the commencement of every paroxysm to take a small dose of acetate of morphia, and to repeat it every hour until the pain abated. She persevered in these means for two months, and experienced considerable relief; but, although the violence of the symptoms was moderated, neither any diminution of the length of the attack, nor any change in the duration of the interval, could be observed.

"Seeing that such was the case, and that the general health of the patient was beginning to suffer from the morphia, it was ordered to be discontinued; and she was directed to take small doses of *strychnia*, for the purpose of removing, if possible, a paralytic affection of the upper eyelid, and of the left side of the face, which had come on during the previous existence of the disease. This course was persevered in, until the convulsive twitchings, brought on by the medicine, became as strong as the patient could bear them, but without producing any effect on the paralysis."

She was ordered to rub *veratria* ointment, (gr. xx, ad ℥i, *gxungies*.) and in about fifteen minutes all uneasy sensations were removed. In two hours they reappeared, but were removed in a few minutes by the same treatment. A return in ten days was dismissed by the same means;

"And from this time the patient had only one or two very slight accessions, but these were at once cut short by the use of the ointment for a few minutes. It is four months since the last attack came

on, and she is at present quite free from the disease. The paralysis of the eyelid also went away."

Several cures of severe neuralgic pains in the back are given, and many slight ones hinted at. The possibility also of curing them in every other part of the body is undoubtingly affirmed, a most modest invitation of the doctor's, which all so affected will of course lose no time in accepting, and thus procure at his hands instantaneous relief. Cures of acute and chronic rheumatism next follow, after which a very frequent and hitherto formidable class of diseases are given,—those of 'paralysis.' We are told—

"In two or three cases the patients had almost entirely lost the power of motion in one side of the body, but recovered it again, by making use of frictions with the ointment over the affected extremities, and more particularly along the course of the nerves."

Among other wonders is the *hopeless* case of a gentleman seventy years old, who had palsy combined with rheumatism, and was unable to move the affected limb. The never-failing veratria ointment soon put motion into this 'ancient of days,' and two hours after the friction he could walk about the room.

We are next presented with an equally interesting group of cases of dropsy, viz., hydrothorax, ascites, anasarca, and dropsy of the ovaries, almost all wonderful cures, not the least among which are two cases of that before indomitable disease, ovarian dropsy.

In dropsy we are cautioned to diminish the strength of the ointment in proportion to the extent of surface over which it is to be rubbed.

"The quantity of urine evacuated by the patient, in some instances, almost exceeds belief."

One instance where sixty-four pints were evacuated in four days, we presume is of this number. In conclusion, Dr. Turnbull tells us how successful the veratria ointment has been in *his* hands in gout, amaurosis, syphilitic pains, &c. In amaurosis, a patient who could only distinguish light from darkness, the eyesight was restored, by one friction, so as to be able to read a small printed book with ease.

Such are the boasted virtues of the veratria as set forth in this production of Dr. Turnbull's. That it will get a fair trial (in spite of the obviously little reliance to be placed on these cases) by a liberal and enlightened profession, we have no doubt, nor have we less that it will miserably fail; nay, we have already

personal knowledge of cases of *tic-douloureux*, rheumatism, and dropsy, where it has done so. But that the end for which the book we have just reviewed was plainly written, will be fully attained, we do not doubt, for an instant, and that the hypochondriac, the nervous, the credulous, the despairing, will crowd to fill Dr. Turnbull's pockets.

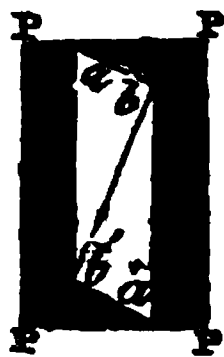
J. H.

SCIENTIFIC INTELLIGENCE.

CHEMICAL AND PHYSICAL SCIENCE.

*On a Method of so far Increasing the Divergency of the two Rays in Calcareous Spar, that only one Image may be seen at a time.**
—Under this title, Mr. Nicol some years ago described in the Edinburgh Philosophical Journal, (No. XI. p. 83,) an instrument which, at first sight, does not seem to possess any superiority over a good achromatised prism of calcareous spar, but which, however, I am convinced from my own experience, does possess important advantages, inasmuch as it throws the images farther asunder than any one I have met with; and I do not even except the parallelepiped, prepared according to Wollaston's directions from two prisms of calcareous spar in which the chief sections are made to cross at right angles.† Nicol's instrument not only answers in place of tourmaline, the green variety of which, as is well known, is difficult to be obtained; but it actually surpasses it essentially, in a property so important in many investigations, that of exhibiting the colour phenomena of crystals and other bodies in polarised light in a manner perfectly pure, and free from foreign tints of colour.

The construction of this instrument will be most easily understood, by reference to the accompanying figure, which represents the instrument as sent from England, and for the use of which I have to thank the politeness of Professor Dove. A section of it is here given of the natural size. $ab, a'b'$, is the principal section of a rhomboid of calcareous spar, which has received the dimensions here represented by cleavage. The long edges, $a b'$ and $b a'$ are the natural obtuse edges of the rhomboid; while the terminal planes $a b$ and $a' b'$, are so cut as to form an angle of 68° at a and a' ; whereas, in their natural state, the angle is 71° . The rhomboid thus modified, is cut through in the direction of the line $b b'$, perpendicular to the principal section and to the terminal planes $a b$ and $a' b'$; and after both the new surfaces have been polished, they are again united by Canada balsam. Both the prismatic halves of



* From Poggendorf's *Annalen der Physik und Chemie*, vol. xxxix. p. 182.

† A similar construction for quartz, described and figured in Herschel's *Treatise on Light*.

the rhomboid* can also, as is more easily effected, be cut in the necessary form from two different pieces, taken either from one or from two crystals. The whole is inserted in the cork PPPP, which has been cut through and again united.

If we look at an object through this rhomboid placed longitudinally, parallel to the edges ab' and ba' , we see only one image in this direction, and that the ordinary one; the extraordinary image comes first into view when we incline very much the long axis of the instrument in relation to the line of sight, and when the power of seeing directly through is not at all disturbed. In this manner the field of sight possesses dimensions which are sufficiently great for optical experiments.

Mr. Nicol has not expressed any opinion as to the cause of this great divergence. It is clear, however, that the chief cause is to be sought in the action of the Canada balsam, which, owing to its refractive power (1,549), and which is that between the ordinary (1,6342), and the extraordinary (1,4833), refraction of calcareous spar, will change the direction of both the rays in an opposite manner before they enter the posterior prismatic half of the combination. Without this action of the Canada balsam, the cutting through and uniting the rhomboid would be of no use, as was at first evident to us, but which has been confirmed by experiment.† Whoever will take the trouble, may calculate accurately the course of both rays, and even of each colour, by means of the already existing data and formulæ.

Upon the plan just described, the mechanician Hirschmann has prepared several of Nicol's rhomboids, which are in all respects the same with that sent from England. Two of them which I myself possess, are as perfect as I could wish them to be. In looking through one of them at a fine line drawn on paper, the line appears quite simple.‡ If we use both, and place them behind one another, directing them upon an object in a horizontal direction, we find, that, when the principal sections are parallel, the object is seen with nearly as much brightness and absence of colour, and with nearly half the distinctness, as when it is regarded with the naked eye; if, however,

* Accurately speaking, the combination is not a rhomboid, but an oblique rhombic prism, since the planes make unequal angles at the three edges of the obtuse solid angles.

† Mr. Nicol was aware, that the increased separation of the images depended on the Canada balsam with which the halves of the rhombic prism was united. If the two parts be united by water, the light sustains total reflection.—*Felt.*

‡ On the other hand, the extraordinary image which is obtained by the strong inclination of the rhomboid receives a mixture of the ordinary one, and the field appears on that account brighter. This is of no disadvantage in the use of the instrument. We also observe a series of feebly coloured edges on the boundary of the two fields of sight, and this is also the case in the rhomboids made in England.

we turn one round until the principal sections are made perpendicular to each other, we then have, at least in the middle of the field, perfect darkness. The weak light which is still visible,* is evidently derived from reflection on the lateral planes, and would undoubtedly be removed if we blackened them. If, while the principal sections cross one another, we insert between the two rhomboids a plate of calcareous spar which has been cut perpendicular to the axis, we obtain a combination entirely formed of calcareous spar, which exhibits the phenomena of coloured rings with the black cross as distinctly as they are obtained by any other method. If we now interpose a plate of mica, of the proper thickness, before and behind the plate of calcareous spar, between it and the rhomboid, so that the axes of these two little plates may cross each other at right angles, and at the same time bisect the right angle between the two principal sections, we then see the coloured rings without the cross having the black centre, and quite similar to the Newtonian rings. An interesting modification of these phenomena has been described by Mr. Airy, and was, at an early period, exhibited to me by Professor Nörrenberg.

It may be remarked, that Brewster had previously employed an analogous method to destroy one of the images in double refracting crystals.† For this purpose, he cuts a prism from a crystal of this nature, (as, for example, calcareous spar, arragonite, saltpetre, carbonate of potash, &c.) and in such a manner, that its angles may be parallel to the optical axis—he leaves two of the planes of the prism rough, and fixes glass plates upon it by means of a suitable substance. If the substance has the same power of refraction as the ordinary ray of the crystalline prism, this ray goes through, since, in relation to it, the planes of the prism are equally polished; but the extraordinary ray becomes scattered on all sides as on a rough plane, and is thus in some measure interrupted. If the substance has the same refractive power as the extraordinary image, the reverse takes place. For example, if we fix the glass plates on a prism of saltpetre, by means of copal balsam, or what is still better, cassia oil, the ordinary ray, whose refraction is 1,511, passes through unweakened, while the other disappears, or appears only as a red light. If, on the other hand, we take for the aniting substance, alcohol, or white of eggs, the extraordinary ray, whose refraction is 1,328, passes through, and the ordinary ray is lost. This plan is decidedly inferior to that of Mr. Nicol, as the image it affords are dull and coloured.—*Edinburgh New Philosophical Journal*, April, 1834.

Professor Faraday's Recent Discoveries.—In the course of his experimental investigation of a general and important law of electrochemical action, which required the accurate measurement of the

* By making the lateral planes rough, which may be done with a file, there is scarcely any lateral reflection.—*Edis.*

† *Phil. Trans.* for 1819, p. 146, and *Edin. Encyclop.* art. Optics, p. 600.

gases evolved during the decomposition of water and other substances, the author was led to the detection of a curious effect, which had never been previously noticed, and of which the knowledge, had he before possessed it, would have prevented many of the errors and inconsistencies occurring in the conclusions he at first deduced from his earlier experiments. The phenomenon observed was the gradual recombination of the elements, which had been previously separated from each other by voltaic action. This happened when, after water had been decomposed by voltaic electricity, the mixed gases resulting from such decomposition were left in contact with the platina wires or plates, which had acted as poles; for under these circumstances they gradually diminished in volume, water was reproduced, and at last the whole of the gases disappeared. On inquiring into the cause of this reunion of the elements of water, the author found that it was occasioned principally by the action of the piece of platina, which had served for the positive pole; and also that the same piece of platina would produce a similar effect on a mixture of oxygen and hydrogen gases obtained by other and more ordinary kinds of chemical action. By closer examination, it was ascertained that the platina, which had been the negative pole, could produce the same effect. Finally, it was found that the only condition requisite for rendering the pieces of platina effective in this recombination of oxygen and hydrogen, is their being perfectly clean, and that ordinary mechanical processes of cleaning are quite sufficient for bringing them into that condition, without the use of the battery. Plates of platina, cleaned by means of a cork, with a little emery and water, or dilute sulphuric acid, were rendered very active; but they acquired the greatest power when first heated in a strong solution of caustic alkali, then dipped in water to wash off the alkali, next dipped in hot strong oil of vitriol, and finally left for ten or fifteen minutes in distilled water. Plates thus prepared, placed in tubes containing mixtures of oxygen and hydrogen gases, determined the gradual combination of their elements: the effect was at first slow, but became by degrees more rapid; and heat was evolved to such a degree, indeed, as frequently to give rise to ignition and explosion.

The author regards this phenomenon as of the same kind as that discovered by Davy in the glowing platina; that observed by Döbereiner in spongy platina, acting on a jet of hydrogen gas in atmospheric air; and those so well experimented on by MM. Dulong and Thenard. In discussing the theory of these remarkable effects, the author advances some new views of the conditions of elasticity at the exterior of a mass of gaseous matter confined by solid surfaces. The elasticity of gases he considers as being dependent on the mutual action of the particles, especially of those which are contiguous to each other; but this reciprocity of condition is wanting on the sides of the exterior particles which are next to the solid substance. Then, reasoning on the principle established by Dalton, that the particles of different gases are indifferent to one another, so that those of one gas

may come within almost any distance of those of another gas, whatever may be the respective degrees of tension in each gas among the particles of its own kind; he concludes that the particles of a gas, or of a mixture of gases, which are next to the platina, or other solid body not of their own chemical nature, touch that surface by a contact as close as that by which the particles of a solid or liquid body touch each other. This proximity, together with the absence of any mutual relation of the gaseous particles to particles of their own kind, combined also with the direct attractive force exerted by the platina, or other solid body, on the particles of the gases, is sufficient, in the opinion of the author, to supply what is wanting in order to render effective the affinity between the particles of oxygen and hydrogen; being, in fact, equivalent to an increase of temperature, to solution, or to any of the other circumstances which are known to be capable of adding to the force of the affinities inherent in the substances themselves.

Some very curious cases of interference with this action of platina and other metals are next described. Thus, small quantities of carbonic oxide, or olefiant gas, mixed with the oxygen and hydrogen gases, totally prevent the effect in question; while very large quantities of carbonic acid, or nitrous oxide gas, do not prevent it: and it is remarkable, that the former of these gases do not affect the metallic plates permanently; for if the plates be removed from those mixtures, and put into pure oxygen and hydrogen gases, the combination of these elements takes place.

The author concludes by some general notice of numerous cases of physical action, which show the influence of certain modifications of the conditions of elasticity at the external surface of gaseous bodies.

The seventh series, which is a continuation of the subject of the fifth, namely, electro-chemical decomposition,* commences with a preliminary exposition of the reasons which have induced the author to introduce into this department of science several new terms, which appear to be required in order to avoid errors and inaccuracies in the statement both of facts and theories. As a substitute for the term *pole*, and with a view to express also a part of the voltaic apparatus to which that name has never been applied, although it be identical with a pole in its relation to the current, the author proposes to employ the term *electrode*. The surfaces of the decomposing body, at which the positive current of electricity enters and passes out, are denominated respectively the *eisode* and the *exode*. Bodies which are decomposable by the electric current are called *electrolytes*, and when *electro-chemically decomposed*, they are said to be *electrolyzed*; the substances themselves, which are evolved in such cases, being called *zétodes*, and the terms *zeteisode* and *zetexode* being applied, accordingly as the substance passes in one direction or the other.

* An abstract of Mr. Faraday's Fifth Series of Experimental Researches in Electricity will be found in Lond. and Edinb. Phil. Mag. vol. iii. p. 460.

The propriety and the advantage of employing these new terms, the author observes, can be properly appreciated only by an experience of their uses and applications in the exposition of the theory of decomposition given in the fifth series of these inquiries, and of that of definite electro-chemical action advanced and supported in the present paper.

The first section of this paper is occupied with the consideration of some general conditions of electro-chemical decomposition. It has been remarked, that elements which are strongly opposed to each other in their chemical affinities are those most readily separated by the voltaic pile; and the discovery of the law of conduction, explained in the fourth series*, has led to a great augmentation of the number of instances which are in conformity with this general observation: but it is here shown, that the proportion in which the elements of a body combine has great influence on the electro-chemical character of the resulting substance; and that numerous instances occur where, although one particular compound of two substances is decomposable, another is not. It appears, that whenever binary compounds of simple bodies are thus related to one another, it is the proto-compounds, or those containing single proportions, which are decomposable, and that the per-compounds are not so.

The second section contains an account of a new instrument devised by the author, for exactly measuring electric currents, and which he terms the *volta-electrometer*. The current to be measured is made to pass through water acidulated by sulphuric acid, and the gases evolved by its decomposition are collected and measured, thereby giving at once an expression of the quantity of electricity which has passed. The principle on which this conclusion is founded is the new law discovered by the author, "*that the decomposing action of any current of electricity is constant for a constant quantity of electricity.*" The accuracy of this law was put to the test in every possible way, with regard to the decomposition of water, by making the same current pass in succession through two or more portions of water, under very different circumstances: but whatever were the variations made, whether by altering the size of the poles or electrodes, by increasing or lessening the intensity of the current or the strength of the solution, by varying its temperature or the mutual distance between the poles, or by introducing any other change in the circumstances of the experiment, still the effect was found to be the same; and a given quantity of electricity, whether passed in one or in many portions, invariably decomposed the same quantity of water. No doubt, therefore, remains as to the truth of the principle on which the *volta-electrometer* acts: but with regard to the practical application of the principle, several forms of the instrument are described by the author, and the mode of employing them, either as the measures of absolute quantities, or as standards of comparison, are fully pointed out.

* See Lond. and Edinb. Phil. Mag. vol. iii. pp. 449, 450.

In the third section of the paper, the primary or secondary character of the bodies evolved at the electrodes is discussed. It is shown that they are secondary in a far greater number of cases than has usually been imagined ; and that laws have been deduced with regard to the ultimate places of substances, from the appearance of the secondary products ; so that certain conclusions, true in themselves, have hitherto been obtained by erroneous reasoning, since the facts which were supposed to support them have, in truth, no direct relation with those conclusions. The methods of distinguishing primary and secondary results from each other are explained, and the importance of this distinction towards the establishment of the law of definite electro-chemical action is insisted upon by the author.

The fourth section is entitled, "On the definite Nature and Extent of Electro-chemical Decomposition," and is considered by the author as by far the most important of this or indeed of the whole series of investigations of which he has now presented the results to the Royal Society. He adverts to the previous occasions on which he has already announced, more or less distinctly, this law of chemical action ; and also to the instrument just explained as one of the examples of the principle about to be developed. He next refers to experiments described in another part, in which primary and secondary results are distinguished as establishing the same principle with regard to muriatic acid ; the results showing, that not only is the quantity of that acid decomposed constant, for a constant quantity of electricity, but that, when it is compared with water, by making one current of electricity pass through both substances, the quantities of each that are decomposed are very exactly the respective chemical equivalents of those bodies. The same current, for example, which can decompose nine parts by weight of water, can decompose thirty-seven parts by weight of muriatic acid, these numbers being respectively the chemical equivalents of those substances, as deduced from the phenomena of ordinary chemical action.

Cases of decomposition are then produced, in which bodies rendered fluid by heat, as oxides, chlorides, iodides, &c., are decomposed by the electric current, but still in conformity with the law of constancy of chemical action. Thus the current which could decompose an equivalent of water, could also decompose equivalents of muriatic acid, of proto-chloride of tin, of iodide of lead, of oxide of lead, and of many other bodies, notwithstanding the greatest differences in their temperature, in the size of the poles, and in other circumstances ; and even changes in the chemical nature of the poles or electrodes, and in their affinities for the evolved bodies, occasioned no change in the quantity of the body decomposed.

The author proceeds in the last place, to consider a very important question with relation to chemical affinity, and the whole theory of electro-chemical action, namely, the absolute quantity of electricity associated with the particles or atoms of matter. This quantity he considers as precisely the same with that which is required to sepa-

rate them from their combination with other particles when subjected to electrolytic action, and he brings many experiments to bear upon this point; describing one, in particular, in which the chemical action of 32.5 parts of zinc, arranged as a voltaic battery, was able to evolve a current of electricity capable of decomposing and transferring the elements of 9 grains of water, being the full equivalent of that number. The relation of electricity, thus evolved, to that of the common electric machine is pointed out in a general way, and the enormous superiority as to quantity, in the former mode of action, is insisted upon. In conclusion, the author refers to a statement which he has made in the third series of these researches,* in which he expresses his belief that the magnetic action of a given quantity of electricity is also definite; and he is now more confident than ever that this view will be fully confirmed by future experiment.—*Lond. and Edin. Phil. Mag.*, April, 1834.

On the Action of Gases Hurtful to Vegetation.—M. Macaire, in making some experiments, an account of which he gave to M. De Candolle, mentioned an accident which had delayed them, viz. the death of several plants by the exhalation of chlorine. M. De Candolle recommended that it should be tried whether this hurtful action occurred during the day or night, observing that chemists who had been consulted on the subject of the exhalations from manufactories, had almost always insisted, that according to their experiments the gas had no action on vegetables. M. De Candolle suspected that these experiments were probably made in the day, a time in which plants do not absorb gases, which would account for the different results obtained. The following experiments were made at his suggestion.

Chlorine.—Plants of euphorbium, mercury, groundsel, cabbage, and sowthistle, which had well taken root, were placed in the morning in a large vessel, in which chloride of lime had been introduced. The roots were steeped outside the vessel: the quantity of chlorine disengaged was far from being sufficient to alter the vegetable tissue. In the evening the plants had not suffered, and the odour of chlorine remained the same.

The same plants, after having passed the night in the same vessel, to which no chlorine was added, were found quite withered in the morning, except the cabbage, which had resisted.

The smell of chlorine had entirely disappeared, and was replaced by that of a disagreeable acid.

This was repeated several times, making the disengagement of chlorine stronger, and the result was the same; and the plants in the day-time withstood a strong atmosphere of chlorine, while a much weaker dose killed them at night.

Nitric acid.—The experiment, began at night, with the vapour

* See London and Edinburgh Phil. Mag. vol. iii. p. 358, et seq.
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of nitric acid, showed that the plants were withered in the morning, and some leaves were rendered brown by the action of the acid. The same quantity of acid was tried in the day-time, and though several leaves were made brown, the others were not withered.

Nitrous acid gas.—This gas appears to be violently poisonous to plants, and at night it kills them in very small quantity; but in the day-time they do not appear to be sensibly altered, although the disengagement of gas is abundant.

Sulphuretted hydrogen.—The results of the experiments with this gas were precisely similar to those of the last. The plants left during the night in the same mixture of gases which did not at all injure them in the light, were quite withered in the morning, and the gas was absorbed. Cabbage only resisted.

Muriatic acid gas.—The results were similar to the last. The plants did not perish in the day-time, even when there was gas enough to render one or or two leaves brown; they were completely dead in the morning, leaving the peculiar smell already mentioned. Cabbage was still an exception.

It appears, then, from these experiments, that several gases are hurtful to vegetation, but that their action occurs only during the absence of light, as M. De Candolle had foreseen.—*Ibid.*

Experiments with Potassium and Sodium.—Dr. Ducatel, Professor of Chemistry in the University of Maryland, after referring to Serullas's experiments on the same subject, (see Phil. Mag. and Annals, N. S. vol. vi. p. 149,) repeated them with the following results:

1. A *mucilage* made with one drachm of powdered gum arabic, and half an ounce of water, will inflame sodium, most probably for the reasons assigned by Serullas, as stated above.

2. On *wood*, sodium most generally inflames in contact with a drop of cold water; the action being at the same time so violent as to cause the globule of metal to roll along the *dry* surface of the table with considerable rapidity, leaving a white streak of caustic soda over its path. This experiment, however, does not always succeed.

3. On a *pane of glass*, sodium will not inflame when the glass is clean and smooth; but any particles of dust adhering to it will cause the firing of the metal with scintillations.

4. On a *metallic* surface the sodium could in no instance be made to inflame.

5. On *charcoal*, which is not mentioned by Serullas, sodium never fails to inflame, with brilliant scintillations. This is the mode which I adopt with most confidence, for firing sodium in contact with cold water. It confirms the truth of the reason given by Serullas, why sodium will not inflame under the same circumstances as potassium; namely, the superior temperature which the latter acquires during its combination with the oxygen of the water: hence the necessity of placing the former on a bad conductor, in order to avoid

the too rapid abstraction of caloric, which prevents a sufficient elevation of temperature for manifesting the phenomenon of combustion.

6. It is commonly stated, that in the decomposition of water by sodium, pure hydrogen is evolved. This is a mistake. A portion of the metal, as in the case of potassium, combines with the hydrogen, as may be shown by the following experiment: Take a globule of sodium, wrap it up in a small piece of paper, and introduce it under a small receiver provided with a stop-cock and jet, filled with water, and standing over the pneumatic trough. The decomposition of the water will be effected as usual, and *sodiuretted hydrogen* will be collected, which (on opening the jet attached to the receiver,) being inflamed, burns with a characteristic bright yellow flame. *Potassiuretted hydrogen*, obtained under the same circumstances, burns with a rose-coloured flame fringed with blue. The potassium, in several repetitions of this experiment, always emitted light; the sodium did not.

7. A globule of *potassium*, placed on a bath of mercury, gradually amalgamates with the latter, without any rotary motion, if the atmosphere be dry; but when breathed upon, it immediately acquires, as observed by Serullas, a very rapid revolving motion, which continues for a long time. The surface of the mercury becomes tarnished, apparently by the accumulation of minute particles of the amalgam formed, which at intervals are seen to emerge from beneath the surface of the mercury, and at some distance from the large globule. The surface of the liquid metal, within a circle of half an inch to an inch in diameter, retains its brilliancy. The minute particles of amalgam, which I suppose to be the cause of the tarnish, seem to be repelled by the large globule of potassium, and occasionally, as new accessions are made to them, they become singularly agitated, exhibiting somewhat of the appearance observed when a drop of vinegar, or of an acid, comes in contact with a drop of water.

8. Small pieces of sodium, projected upon a bath of mercury, were not found to exhibit the phenomena indicated by Serullas; that is, they were not thrown off with explosions accompanied with light and caloric. The effects are, however, curious. The amalgamation of the sodium takes place slowly, without any rotary motion; although sometimes, when breathed upon, a motion of short duration is induced. When several pieces are put upon the bath at the same time, they show no disposition to come together, but rather the contrary. But when one piece is pushed towards another, there appears to be, within a certain distance, an attractive force exerted, which is immediately succeeded by a repulsive one of some comparative energy. Many pieces being accumulated in a small space, they become violently agitated, as if alternately attracting and repelling each other, until they finally separate.—*Ibid.*

On the Influence of successive Impulses of Light upon the Retina, by Sir David Brewster.—From the remarkable experiment of

Mrs. Griffiths, described in a former number,* it appears that the reticulated structure of the retina may be rendered visible by throwing light suddenly on the closed eye, when in a state of repose, and especially in the morning, before the retina has been subjected to the action of any other light.

In repeating this experiment, I have found that a remarkable structure may be exhibited at any time, and whether the eyes are open or shut, by subjecting the retina to the action of successive impulses of light. If, when we are walking beside a high iron railing, we direct the closed eye to the sun so that his light shall be successively interrupted by the iron rails, a structure resembling a kaleidoscopic pattern, having the *foramen centrale* in its centre, will be rudely seen. The pattern is not formed in distinct lines, but by patches of reddish light of different degrees of intensity. When the sun's rays are powerful, and when their successive action has been kept up for a short time, the whole field of vision is filled with a brilliant pattern, as if it consisted of the brightest tartan, composed of red and green squares of dazzling brightness. The green colours prevail chiefly at the centre, corresponding to the *foramen centrale*, and here we observe the dark-lined *network* pattern which I have described in No. 20,† and which is totally distinct from the reticulated structure noticed by Mrs. Griffiths. The brilliancy of the spectrum thus produced, and the beauty of its colours, exceed any optical phenomenon which I have witnessed, and so dazzling is its effect that the eye is soon obliged to withdraw itself from its overpowering influence.

The very same phenomena may be seen by looking at the sun through the distended fingers when they are made to move backwards and forwards, or rather from right to left, and from left to right, in front of the eye.

The colours of the spectrum above described have their origin in the red light transmitted through the eyelids, the green tints being the accidental or complementary colour of the red; but the phenomenon may be seen in a great measure without colour by opening the eye, and interposing between the eye and the sun any white transparent ground, such as thin white paper or ground glass, or by directing the eye immediately to a bright sky, or to the ground when covered with snow. In these different forms of the experiment the effect varies greatly with the intensity of the light and the state of the eye, but the following general description of the phenomena will be found tolerably correct. In order to make the light produce a series of successive impulses on the retina, and on the same parts of it, I look through the openings of the resolving disc of the phenakistiscope with one eye, and fix it steadily upon the same point of the luminous ground.

* See London and Edinburgh Philosophical Magazine, vol. iv. p. 43.

† Ibid, p. 119, note.

When the disc revolves with great velocity, a very faint and uniform light is seen over the whole luminous surface. As the velocity diminishes, the light becomes less uniform, and a flickering or wavering motion commences. Patches of a *bluish-purple* colour appear in different parts of the field, forming a sort of network, the intersections between the meshes of which are of a faint *lemon-yellow* colour, the accidental colour of the *bluish-purple*. The pattern of this network is related to the centre or point on which the eye is fixed, and seems to belong either to a hexagonal or octagonal division of the circle. The centre of the pattern, corresponding to the *foramen centrale*, is a square or lozenge, one of whose diagonals is vertical; but as the differently coloured patches or elements of the pattern are constantly changing their colour, their intensity of light, and even their form, owing to the unsteadiness of the eye and the revolving disc, I have never been able to draw the pattern, or to trace how the patches or interstices of the network spring from the sides and angles of the central lozenge. That the reticulated structure is related to this central square or to the central foramen of the eye is unquestionable; and I have no doubt that observers who have younger eyes than mine, and who shall have the courage to repeat the experiments with the direct light of the sun, and with a disc having narrow slits, and revolving upon a fixed axis so as to have its velocity uniform, will be able to obtain an accurate representation of the pattern in question.

Within and around the central lozenge, is seen with great distinctness the dark-lined network pattern already mentioned, and apparently unconnected with the larger pattern. As the spaces, however, or patches, which compose the larger pattern diminish in size towards the centre, it is possible that the dark-lined network, with dark specks in the centre of the figures, and having all the regularity of a geometrical figure drawn with ink, may be the central part of the larger patterns seen more distinctly by direct vision; but I cannot admit this notion, because under favourable circumstances a similar dark-lined pattern, with extremely small spaces between the meshes, appears throughout the rest of the field of view, especially in the external part of it where it first begins to show itself.

The colours which appear in the principal pattern are chiefly *bluish-purple*, and its complementary colour *lemon-yellow*, but as the former increases in depth or approaches to blackness, the latter becomes more white. These different colours sometimes appear in the different patches of the pattern, and sometimes they appear in succession over a considerable part of the field. They are, however, most distinctly seen in the central lozenge, the inner part being sometimes *purple* and then *yellow*, while the outer part of it is first *yellow* and then *purple*. The central lozenge is sometimes marked out by whitish, and sometimes by greenish light, and I have frequently seen in its centre a *blush-red* of a very rare tint. The suc-

cession of colours in the lozenge is very beautiful, each colour advancing to the centre, replacing that which preceded it, and then disappearing.

The cause of the colours themselves is obvious. The action of white light on the retina renders it first insensible to the red rays, and consequently a white object or ground appears *bluish purple* or *blue* in solar light, and *green* in candle light, the colour varying with the intensity of the exciting light, and with the distance of the image of the white object from the excited point. The other colour which appears in the preceding experiments is a faint *lemon-yellow*, which is the complementary colour of the *bluish purple*. It deserves also to be noticed, that these colours are the very same as those produced by the action of light falling on the retina at a distance from the axis of vision. When we look, for example, indirectly, or rather obliquely, at a candle for some time, the image of the candle itself becomes *bluish purple* surrounded with a nebulosity of *yellow* light, the accidental colour being the invariable companion of the primitive one.

In order to explain why no colours appear during a very rapid rotation of the disc, and why the primitive and the accidental colour succeed each other in the pattern, let us call

T the time in which the disc revolves,

n the number of apertures in its margin,

D the duration of the impression of direct light, and

d the duration of its complementary colour.

It is obvious that $\frac{T}{n}$ will be the time which elapses between each

consecutive impulse of light on the retina, or the time during which the eye has the opaque part of the disc opposite the pupil. When

$\frac{T}{n}$ is very small, or the velocity very great, or when $\frac{T}{n}$ is very much

less than D , (D is \approx eight-thirds, or nearly one-eighth of a second,) the ground will be uniformly luminous, because the direct impression of the one aperture has not begun to fade away before the succeeding

aperture makes a new impression. When $\frac{T}{n}$, however, is nearly

equal to D , the impression of the direct light is nearly gone, and hence arises the flickering or wavering appearance of the luminous

ground, which becomes a maximum when $\frac{T}{n} = D$; for when this

takes place, the direct impression of the one aperture is just gone

before the other aperture renews it. When $\frac{T}{n}$ is greater than D , the

accidental colour of the direct impression begins to show itself; and

* See Lond. and Edinb. Phil. Mag., vol. iii. p. 169.

when $\frac{T}{n} = D + \frac{d}{2}$, the accidental colour will be about its brightest, and will be seen to succeed the direct impression, the latter being now *bluish purple*, and the former *lemon-yellow*. When $\frac{T}{n} = D + d$, the opaque space between the apertures will begin to be visible, and the phenomena will disappear.

As the reticulated pattern is marked out by different colours and even by the same colour in different states of intensity, it follows that different parts of the retina have different degrees of sensibility to light. The lines which form the network are probably thicker than the interstices between them, and consequently less susceptible to luminous impressions. In like manner the interstices nearest to the *foramen centrale* are probably thinner than those more remote, and hence it is easy to understand why they exhibit a greater sensibility and a more rapid change of colour. If these views are correct, we not only obtain a general explanation of the phenomena which we have described, but of many others which have hitherto perplexed the optical physiologist, and among these we may enumerate the phenomena of oblique vision, and the superior distinctness of objects when they are seen directly along the axis of the eye.

In a former paper I had occasion to mention a very remarkable fact, which I had long ago discovered, that the intensity of a given light may be increased physiologically by causing it to act upon the retina by successive impulses of a given duration. Those who may repeat the preceding experiments will obtain an ocular demonstration of the truth of this new property of light. The maximum physiological intensity seems to take place when $\frac{T}{n}$ is nearly equal to $D + d$.
—*Ibid.*

NATURAL HISTORY.

Remarkable Skulls found in Peru.—In the Second Part of the Fifth Vol. of the *Leitschrift für Physiologie*, Tiedemann gives the following short but interesting notice :

During my late visit to Paris, I saw, in the Museum of Comparative Anatomy, at the Jardin du Roi, several skulls brought by Mr. J. R. Pentland from Peru, where he found them in sepulchres of a very ancient date. These skulls are remarkable for their unusually great length, the axis from the forehead to the occiput being much longer than what is observed in any other skulls I have seen, while the lateral axis is proportionably shorter, in consequence of which they seem compressed at the sides. The face is exceedingly projecting, and the forehead very retreating, so that the facial angle of

Camper is smaller than in any known race of man. The os frontis is continued far backwards towards the vertex, and is very long, narrow, and flat. The parietal bones look partly backwards, and where they join the frontal bone make a remarkable arch or protuberance. The occipital foramen is large, and its plane looks not downwards and forwards, but somewhat backwards. The zygomatic processes are not prominent. Mr. Pentland has given me the following information concerning these extraordinary skulls:

"The singularly shaped skulls which excited your attention, I obtained in 1827, in the Province of Upper Peru, now called Bolivia. They seem to belong to an extinct race of mankind. I found them in the ancient graves called Huacas, in the great alpine valley of Titicaca, which is likewise remarkable for being the country in which civilization, planted by the Peruvians, flourished to a degree unrivalled among the other tribes of the new world. These sepulchres have the form of high round towers, and in some places are constructed of enormous masses of masonry. The stones are very carefully and skilfully arranged, in a manner similar to that observed in the old structures of Greece and Italy, named by our antiquarians Cyclopean. I have met them only in the valley of Titicaca, which extends from the seventeenth to the nineteenth degree of latitude (south), and on the skirts of the Andes which form that valley. They occur in the greatest abundance in the Provinces of La Paz, Oruro, Pucallpa, and Carangas. I examined several hundreds of these sepulchres, and in all of them found human skeletons, and in all the skull had the same singular shape. The skeletons are in a state of excellent preservation, a circumstance attributable to the great dryness of the climate, the country being situated about two thousand toises above the level of the sea. The skeletons belonged to persons of all ages from the youngest child to the oldest man. All the heads, young and old, had the same form, from which I conceive that it may be with justice inferred that their peculiar shape was not artificially caused by pressure, as is the case with the Caribs, and some other of the barbarous tribes of the new world.* The heads presented to the French museum were selected from a great number, and were found in the following places, viz.: the island in the lake of Titicaca; Chunguza, Tiaguanaca, Licasica, Tolapalea, and Lennas. The present inhabitants of the interior of Peru belong to the same race which peopled the remainder of the continent of South America, and which is distinguished by a copper colour of the skin, a thin, scattered beard, straight strong black hair, and a prominent nose. I am of opinion that the present inhabitants of Peru are derived from an Asiatic stock, the last colony of whom migrated probably about the twelfth century. From these latter settlers sprung the dynasty of the Incas or Ingas, which dynasty was overthrown by the Spaniards, after it had lasted about twelve generations. The Asiatic emigrants had gradually spread themselves over the western coast of

South America, conquering, as they spread, the natives of the country; and it is to the latter, in all probability, that the skulls in question belonged. Many sepulchres of the present race of Peruvian Indians occur along the coast of the Pacific Ocean, the skulls found in which agree in every respect with the form of that race, but in no instances do they possess the peculiar characters of those found in the interior. A careful examination of these skulls has convinced me that their peculiar shape cannot be owing to artificial pressure. The great elongation of the face and the direction of the plane of the occipital bone are not to be reconciled with this opinion, and therefore we must conclude that the peculiarity of shape depends on a natural conformation. If this view of the subject be correct, it follows that these skulls belonged to a race of mankind now extinct, and which differed from any now existing."

"From this very valuable communication of Mr. Pentland," says Tiedemann, "we are justified in inferring that the continent of America was inhabited by aborigines, who belonged to a race of mankind differing from every other in several important particulars, viz.: a great projection of the face, great length, flatness, and narrowness of the os frontis, prominence of the back part of the cranium, an exceedingly small facial angle, and an unusual direction of the plane of the occipital foramen. This race has in the course of ages become extinct, and was probably extirpated by their foreign conquerors."

Continuation from Professor Scouler on the above subject.—The following observations on the above notice concerning the Peruvian skulls, is well worthy of attention, as coming from the pen of an eminent naturalist, Professor Scouler, who has visited some of the countries in question, and has himself brought to Europe similar skulls; and on whose recent appointment to the chair of Natural History in the Royal Dublin Society, we most sincerely congratulate the Irish public.

"The Peruvian cranium described by Tiedemann in the preceding article, possesses so very remarkable a configuration, that we would be tempted to adopt his opinion that it belonged to an original and primitive race, if we were certain that its form had not been produced by artificial means. If we remember that the practice of deforming the head by means of pressure has been very general throughout America, and the result has been the production of crania as anomalous as those of the ancient Peruvians, we will rather admit that in this instance also compression has been employed. We are aware that the possibility of deforming the cranium by the application of continued pressure has been denied by able anatomists; but it is unnecessary to examine their reasonings in a case where we can appeal to positive facts. That the Caribs of St. Vincent's flattened the heads of their children is well known; and an inspection of Blumenbach's engraving of a Caribbean skull, will convince any one of the great

amount of deformity which may be produced.* The same custom prevailed in Carolina according to Adair, and at the present moment it is practised by the Indians inhabiting the banks of the Columbia river and Noetka Sound; of the authenticity of this fact, as far as regards the Indians of the Columbia river, we can entertain no doubt, as we have enjoyed ample opportunities of witnessing every stage of the process, and have in our possession a complete series of skulls, in some of which the deformity is as great as in the Peruvian skull figured by Tiedemann.† Among the Columbian tribes, the child, immediately after birth, is put into a cradle of a peculiar construction, and pressure is applied to the forehead and occiput. After the head has been compressed for several months, it exhibits a most hideous appearance; the antero-posterior diameter is the smallest, while the breadth from side to side above the ears is enormous, thus reversing the natural measurements of the cranium. As the individual increases in years the deformity becomes less, but even in the adult persons it is very great; from the excessive depression of the forehead the eyes appear as if turned upwards, a circumstance which gives a very peculiar physiognomy to the Indian. The process is slow and gentle, so that the child does not appear to suffer in any way from so unnatural a process, nor do the intellectual qualities of the individual appear to be in any degree affected by it; on the contrary, a flat head is esteemed an honour, and distinguishes the freeman from the slave.

“These circumstances are sufficient to establish the fact, that the human cranium may be distorted by artificial means, and thus render it probable that the skulls of the ancient Peruvians may have been disfigured by the same process. This opinion is greatly strengthened by other circumstances. Blumenbach has figured a deformed and compressed Peruvian cranium from Quilca,‡ the form is different from that of the skull represented by Tiedemann, and from those of the Indians of North West America, but different modes and degrees of compression will produce different kinds of deformity. We have in our possession a skull in which the pressure has been applied diagonally from the left half of the frontal to the right half of the occipital bone. In addition to these facts, we have the testimony of historians and travellers, that it was the practice in Peru to compress the heads of the children. The following authorities, as quoted by Blumenbach, are sufficient to establish this point. De la Condamine informs us that the custom prevails in South America, and that it was known to the Peruvians, and in the year 1585, the synod of Lima prohibited the custom under the pain of ecclesiastical punishments. The synod alludes to the practice as universally prevalent in Peru, and that it

* Blumenbach, *Decades Craniorum*, tab. i. an.

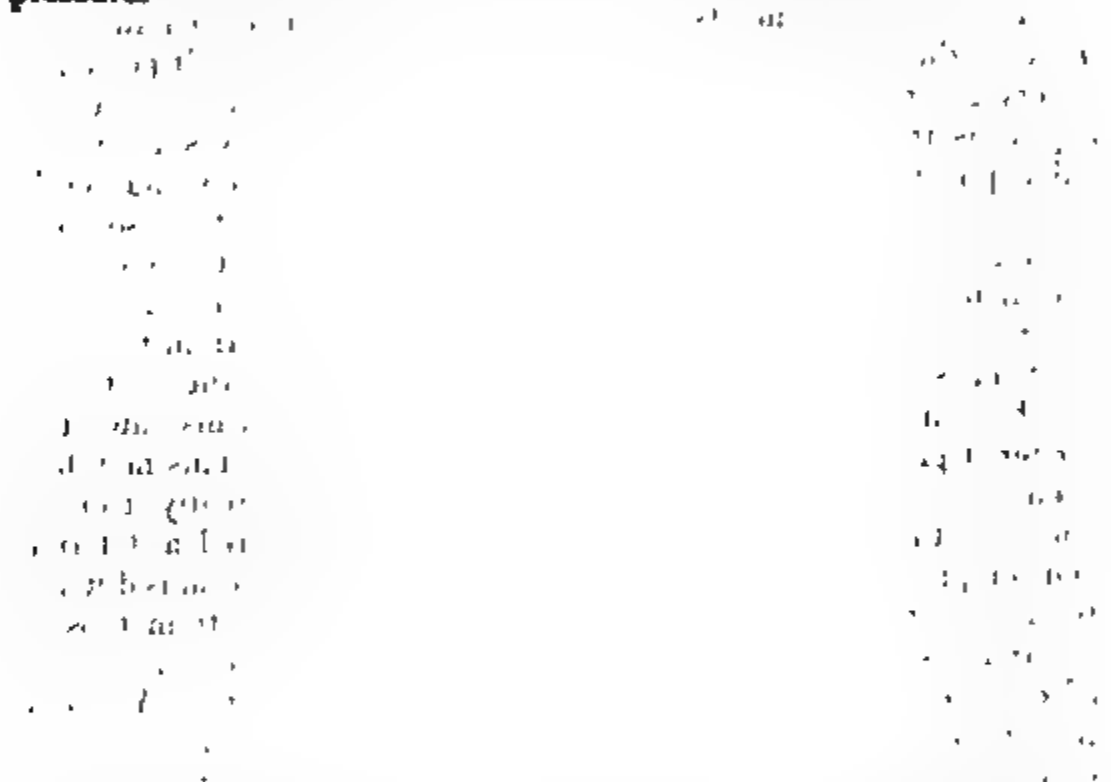
† Scouler, *Zoological Journal*, where an account of the mode of compression, and figures of the compressing machine, and of an adult and infant skull, are given.

‡ Blumenbach, *Decades Craniorum*, tab. xlv.

has fallen into disuse since the arrival of the Spaniards in that country.

"These facts are sufficient to prove the possibility of modifying the human cranium by means of pressure; that the custom was very general throughout North and South America, and that the practice prevailed among the ancient Peruvians; consequently, it is more probable that the ancient skulls of Titicaca owed their strange configuration to a process which we know is capable of explaining the phenomena, than that they constituted an original race, a circumstance of which we have no other evidence than that derived from the shape of the cranium.

"The accompanying sketch of the infant skull of the Columbian tribes, will give an accurate idea of the form of the head produced by pressure."



Mode of Flattening the Head among the Tribes on the Columbia.—The abominable custom of flattening their heads prevails among them all. Immediately after birth, the infant is placed in a kind of oblong cradle formed like a trough, with moss under it. One end, on which the head reposes, is more elevated than the rest. A padding is then placed on the forehead with a piece of cedar bark over it, and by means of cords passed through small holes on each side of the cradle the padding is pressed against the head. It is kept in this manner upwards of a year, and the process is not I believe attended with much pain. The appearance of the infant, however, while in this state of compression, is frightful, and its little black eyes, forced out by the tightness of the bandages, resemble those of a mouse choked in a trap. When released from this inhuman process, the head is perfectly

* Blumenbach de Genere Humani Varietate Natura, p. 220.

flattened, and the upper part of it seldom exceeds an inch in thickness. It never afterwards recovers its rotundity. They deem this an essential point of beauty, and the most devoted adherent of our first Charles never entertained a stronger aversion to a Round-head than these savages.*

They allege, as an excuse for this custom, that all their slaves have round heads; and accordingly every child of a bondsman, who is not adopted by the tribe, inherits not only his father's degradation, but his parental rotundity of cranium.—*Cox's Travels on the Columbia River.*

Rapidity of Vegetable Organization.—The vegetable kingdom presents us with innumerable instances, not only of the extraordinary divisibility of matter, but of its activity, in the almost incredible rapid development of cellular structure in certain plants. Thus the *Bovista giganteum* (a species of fungus) has been known to acquire the size of a gourd in one night. Now supposing, with Professor Lindley, that the cellules of this plant are not less than the $\frac{1}{10}$ th of an inch in diameter, a plant of the above size will contain no less than 47,000,000,000 cellules; so that, supposing it to have grown in the course of twelve hours, its cellules must have been developed at the rate of nearly 4,000,000,000 per hour, or of more than sixty-six millions in a minute! and when we consider that every one of these cellules must be composed of innumerable molecules, each one of which is again composed of others, we are perfectly overwhelmed with the minuteness and number of the parts employed in this single production of nature.—*Prout, Bridgewater Treatise.*

Introduction of Frogs into Ireland.—It is not generally known that the introduction of frogs into Ireland is of comparatively recent date. In the seventeenth number of the Dublin University Magazine, there is a quotation from the writings of Donat, who was himself an Irishman, and Bishop of Fesculæ, near Florence, and who, about the year 820, wrote a brief description of Ireland, in which the following passage occurs:

“Nulla venena nocent, nec serpens serpit in herba.
Nec conquesta canit garrula rana lacu.”

At this very hour,” says our respected contemporary, “we have neither snakes nor venomous reptiles in this island, and we know, that for the first time, frog spawn was brought from England, in the year 1696, by one of the Fellows of Trinity College, Dublin, and placed in a ditch in the University park or pleasure ground, from which these very prolific colonists sent out their croaking detach-

* Dr. Swan, on examining the skulls I had taken, candidly confessed that nothing short of actual demonstration could have convinced him of the possibility of moulding the human head into such a form.

† Introduction to Botany, page 7.

ments through the adjacent country, whose progeny spread from field to field throughout the whole kingdom. No statue has yet been erected to the memory of the natural philosopher who enriched our island with so very valuable an importation of melodious and beautiful creatures."

We may state, however, that we have learned from good authority, that a recent importation of snakes has been made, and that they are at present multiplying rapidly within a few miles of the tomb of St. Patrick.—Ed.

ANATOMY AND PHYSIOLOGY.

Remarks on an Anastomosis recently observed outside of the Liver, between the Vena Portæ and the Vena Cava inferior, by Professor Retzius, of Stockholm.—In the spring of 1832, being employed in making preparations of the venous system, I met with various venous branches, which, coming from the duodenum, left colon, and rectum, formed a communication with and united themselves to the vena cava inferior. Having put a ligature on the vena portæ, where it enters the liver, I injected this vein below the ligature with an injection of a particular colour: in the same manner I filled the vena cava inferior, with an injection of another colour. In displaying the ramifications of the injected veins, I observed several small branches of the same coming from the duodenum, and emptying themselves into the neighbouring trunk of the vena cava inferior. Other venous branches coming from the left portion of the colon, joined the emulgent vein of the left kidney. Further, I observed, that some veins coming from the rectum, united themselves to the internal venous network of the organs of generation. These latter were the largest and most numerous. As the vena portæ and the vena cava were filled with differently coloured injections, it was easy to distinguish the anastomosing branches by which they were connected. These observations, made on the body of a boy aged five years, were confirmed in two other subjects of the same age. A very minute examination disclosed the existence of a thick network of very minute venous twigs in the cellular membrane outside the peritoneum. The veins with which this network communicated were connected on the one hand with the vena portæ, on the other with the vena cava inferior. When the peritoneum is fresh, thin, and transparent, the largest of these vessels can be seen and traced with the naked eye. When the injection succeeds well these vessels may be seen on the posterior parietes of the abdomen proceeding to join the veins of the colon. They at the same time form numerous anastomoses with the veins of the kidneys, branches of the pelvic veins, as well as with other veins which empty themselves into the inferior cava. From this it appears that in the human body a communication exists between the vena cava inferior, and the vena portæ, which, so far as I am aware,

has not yet been remarked by any writer. It can be most satisfactorily demonstrated by simultaneously injecting the trunks of the vena portae, and of the vena cava inferior. The latter may be often successfully injected, even to its minutest branches, for its valves do not constantly present an obstacle capable of impeding the progress of the injected fluid. In this manner I once succeeded in filling the veins of the rectum with an injection from the vena cava inferior. The first injection passed from the cava chiefly into the venous network of the cellular and muscular tunics of that intestine, while its mucous membrane, on the contrary, was injected with the fluid that had been thrown into the vena portae. I have made similar observations on the duodenum, and on some portions of the colon. It is more than probable that a similar distribution of vessels occurs in all vertebrated animals, in many of whom indeed Jacobson* has already announced its existence.

Professor Retzius is of opinion, that these numerous venous anastomoses in the various networks of vessels he has observed in the peritoneum, afford an explanation of the relief obtained by the application of cupping glasses or leeches to the abdominal parietes, in cases where the bowels or the peritoneum are inflamed; for the blood thus drawn must influence not merely the circulation in the peritoneum, which lines the abdominal parietes, but also the system of the vena portae.—*Zeitschrift für Physiologie*, B. 5. H. i. p. 105.

Lymphatics in the Umbilical Cord.—In the last number of this Journal, we inserted a notice of Fohman's discovery and successful injection of lymphatics in the placenta and funis of the human race. More recently a similar investigation has been undertaken; and with an equally satisfactory result, by Dr. Montgomery of this city, who has succeeded in injecting with mercury great numbers of these delicate vessels running along the cord, and, for the most part, following the spiral course of the umbilical arteries.

Difference between Growth and Development.—In treating of precocious growth, M. Isidore St. Hilaire draws a nice line of distinction between growth and development in the following terms:

“*Growth* is a gradual increase in each of the parts of the body, independent of full change in their number, their structure, and their functions. *Development* consists, on the contrary, essentially in a modification, or a complete change more or less manifest.

“In the phenomena of simple growth, all the conditions of the existence of organs are preserved, except their volume. Every phenomenon of true development, on the contrary, allowing for the difference in the extent and intensity of the change, resembles those

* *De Systemate Venarum Peculiarium in perneis Animalibus observato.*—Hobniz, 1821.

remarkable transformations and metamorphoses of the embryotic age, and of the foetal life.

"The eruption of the teeth of the first dentition; that of the permanent teeth, and finally the period of puberty, mark in man and the animals which approximate to him, three principal epochs of development, after each of which the general growth is commonly retarded, in a more or less marked degree. Thus, increase of stature is very rapid up to the eruption of the primary teeth, it is still considerable, although less, until the second dentition, and from this period to puberty, its progress is still slower; after this last period it is much less; and almost always becomes nearly imperceptible, or ceases altogether when puberty is completed.

"Growth and development, although essentially distinct, are then intimately connected. A certain degree of growth in all the organs brings in its train a new development. Every development, on the other hand, marks the termination of one period of growth and the commencement of another, during which the organs grow more slowly, as if the newly developed apparatus had appropriated to itself, or had concentrated on it a great part of the formative force, before divided with more equality among all the organs, (in more exact terms, and banishing this abstract language, too long used by anatomists), because the newly developed apparatus, by the increased activity of its own nutrition, necessarily diminishes the activity of general nutrition." pp. 188, 189.

The author then proceeds to show, that as developments indicated by the eruption of the primary and permanent teeth, and the perfection of the genital organs, exert a controlling influence on the growth of the human body, and that of other animals resembling man in their organization; so on the other hand, growth influences development to a considerable extent: thus, when an infant grows rapidly, the primary teeth appear early; their eruption occurring shortly after birth, or even preceding it; on the contrary, when the general growth of an infant is slow, the eruption of the teeth is proportionably retarded. The same remark applies equally to the influence of rapid or slow growth in hastening or retarding the eruption of the permanent teeth, and the appearance of the signs of puberty. The foregoing observations refer to the mutual influence which growth and development exert on each other, while the normal relation is maintained between them: but this relation is subject to occasional derangement, as stated in the following passage.

"When a child increases in size prematurely, and attains a considerable stature at an early age, the first symptoms of puberty always appear unusually early. One of two things may then happen: either the development of the genital organs may proceed slowly, and even be suspended, notwithstanding its precocious commencement, or it may proceed rapidly to completion. p. 192.

"In the former case, the development of the genital organs being slow and incomplete, the general growth continues to proceed rapidly; the stature, already considerable before the appearance of the first

signs of puberty, increases still after their appearance, and becomes gigantic. Hence the small size and inactivity of the sexual organs in giants, and consequent impotence." p. 192.

The author next points out the effects produced in those precocious individuals, by suspension of the general growth, and a rapid development of the genital organs. Such persons have the period of puberty early completed, and do not exceed the middle size, or even fall short of it.

It is further remarked, that precocious growth of the body, generally, is more common in males, and precocious development of the sexual organs, or of the period of puberty, in females. Another physiological fact in the history of precocious individuals, is that after the eruption of the teeth, far from having been precocious, was unusually slow. The reverse, however, has been observed.

Precocious children have in general had voracious appetites. Sometimes the growth became very rapid during a fit of illness. In girls it has supervened on a suppression of the menses. But it often commences from the time of birth, without appearing to depend on any obvious cause.

Precocity of growth does not usually affect many individuals of the same family; nor does it seem to be hereditary. The generality of precocious children have died at an early age. A few of the more remarkable cases of precocious development on record are quoted, in which the characters above enumerated were manifest, and for the details of which we must refer to the work itself.

In the cases of precocious development of the genital organs heretofore related, the accompanying state of the cerebellum has not been so particularly stated as to afford any argument for or against the doctrine of Gall, as to the connexion between this part of the brain and the venereal appetite. On this point the author remarks, however, "according to the doctrine of Gall, we should not expect that all children remarkable for their growth and precocious puberty, would present a more than ordinary development in the cerebellum; for children, who have prematurely exhibited marks of puberty, experience no venereal desire, while other individuals have been seen having no marks of puberty, and presenting nothing particular in the state of their genital organs, yet tormented with very ardent desires." p. 203.

The section on precocious growth, is closed with a short notice on the anomalies of size observed in the foetus at La Maternité, where it appears that tables are kept, in which it is recorded that the foetus at birth has varied in length from eighteen to twenty-five inches.

PATHOLOGY AND THERAPEUTICS.

Tuberculous Diseases of Monkeys.—Dr. Reynaud, one of the most accurate of the French pathologists, lately read a memoir before

the *Academy of Medicine*, Paris, on the tuberculous affection in monkeys, compared with the same affection in man. The following are the principal facts established by this interesting piece of comparative pathology, to which we refer those who wish for more extensive or more exact information on the subject. It is published in the *Archives de Médecine*, vol. xxv, pp. 149—171, and pp. 301—326.

1. Nearly all the quadrumana brought to Europe die tuberculous. This has been attributed to confinement and to change of climate; but till we know of what diseases they die in their wild state and in their native country, such opinions must remain conjectural.

2. The tuberculous deposition is more abundant, and at the same time is diffused through a greater number of organs, than in man.

3. The organs which the deposit affects are not the same. In the monkey it is generally found in the *kidneys*, the *spleen*, and the *liver*; often in the heart, very rarely in the mesenteric glands, or on the peritoneum—never in the small intestine. The reverse of this occurs in man. The lung is the most frequent seat of the disease in both; but in the monkey the spleen is affected to almost the same extent, and the tubercles soften with almost equal rapidity.

4. In all these respects the tuberculous affection in children forms a sort of intermediate link between the disease in the human adult, and in this family, inferior to ourselves in the zoological scale.

The following facts will serve at once to illustrate, confirm, and limit these general principles.

Dr. Reynaud examined more than twenty monkeys that died at the Garden of Plants in Paris. All with one exception died phthisical. They had tubercles in the lungs, and likewise in several other organs. Fifteen cases of which he took notes, form the basis of his observations. The species examined were various; but neither this, nor sex, nor age, nor time of residence in Europe, influenced the uniformity of the lesion, which was constantly tuberculous.

In the fifteen cases one was not tuberculous; fourteen had tubercles in the lungs; in three they were confined to the lungs; in one they were extensively developed in these organs, while there were a few tuberculous points in the mesenteric glands. In the ten remaining cases, tubercles existed in several organs simultaneously.

Dr. Reynaud had rarely an opportunity of examining the osseous or the nervous systems. The liver was fat (*gros*) in no instance; in man, on the contrary, this lesion occurred in 40 out of 120 cases observed by Louis.* Pericarditis was met with in four of the cases; it was developed subsequently to the tubercles, and appeared to be regulated by the same laws as pleuritis in the phthisical.

The lungs were in several cases entirely or in part converted into tuberculous matter; none of the tissues remained; all had undergone the same transformation. Ulcerations were discovered twice in the

* *Phthisie Pulmonaire*, pp. 115, 116.

larynx; never, in the trachea or its two divisions. In man, ulcerations of these parts occur in the majority of cases. Louis attributed them to the inflammation excited by the matter expectorated. Now it is singular, that in the monkey the cough is always dry, and unaccompanied by expectoration.

The bronchial glands were always more or less implicated in the tuberculous affection. In one case their enlargement had completely obliterated the left bronchus, and thus obstructed respiration in the corresponding lung, which, as well as the side of the thorax, was much contracted.

The spleen in two cases contained a few crude tubercles. In the six remaining cases it was completely stuffed with this morbid product, enlarged, deformed, and often adherent to the peritoneum. The blood in the cells formed reddish clots, in the midst of which were tuberculous points. Tubercles in various stages of development replaced the tissue of the organ; this deposit was sometimes contained in caverns lined with a false membrane. In one case the tubercles were isolated and crude in the lungs; in the spleen, on the contrary, they were large, and softened in the centre, thus constituting a true splenic phthisis.

Dr. Reynaud has made it highly probable that these alterations take place in the blood contained in the cells, and do not commence in the tissue of the spleen itself.

The stethoscope and percussion enable us to detect the changes which take place in the thorax of these animals, as the following short abstract of a case will show:

A Papion, aged about five years; in Europe eight months, coughed, and had dyspnoea. The left side of the chest was clear on percussion, the right was dull. The respiration on the left side was vesicular, mixed with râle; on the right it was cavernous, and gurgling. There was cough, but no expectoration. The pulsations of the heart could scarcely be counted.

Inspection.—On the left side there was a recent effusion of blood, and false membranes in the pleura. Some crude and softened tubercles were found in the lung. The right lung contained a great number of tubercles isolated, and in irregular masses. One of the inferior lobes was converted into a large, purulent, tuberculous mass. The bronchial glands were enlarged and tuberculous. The spleen contained a great many tubercles in various stages of softening, and caverns of various sizes. The liver contained a few yellow crude tubercles. In the kidneys there were one or two. The epiploon was covered with little semi-transparent granulations. The heart, pericardium, stomach, and intestines, were healthy. The mesenteric glands unaltered.

According to Louis and Dr. Reynaud's calculations, the left lung is more frequently tuberculous than the right, and to a greater degree; females are more subject to phthisis than males—children than adults; and in the division of animals which approaches nearest to ourselves,

tubercularization is at its maximum of frequency, extent, and diffusion. "Then," says Dr. R., "whether we consider man relatively to the lung, apparently most disposed to tubercles, and belonging to that side of the body which, compared with the other, seems inferiorly developed;—whether we regard sex, age, or species,—tubercles follow one constant order in their development—that of organic degradation.—*Lancet*, April 26, 1834.

Irregular Formation of the Breasts.—Niedemann remarks on this subject: "The human female presents irregularities of the breast not less frequently than other mammalia. In addition to the instances already recorded, I myself have observed the following:—In 1824, Maria R., born in Hagenfeld, near Pforzheim, was admitted into our Lying-in Hospital. Her left breast was furnished with two nipples, each of which was enclosed within a separate areola. The supernumerary nipple was situated about three finger's breadth beneath the other. This breast on the whole was not larger than that of the right side. She was delivered of a son, and milk came in as great abundance from the supernumerary as from the other nipples.

In the year 1825, the body of a girl was brought into our dissecting room; she was about twenty years old, her breasts were beautifully formed, and well developed, but each furnished with two fully formed nipples, surrounded by a common areola. In 1829, the wife of a Dutch soldier named Walter, called for the purpose of consulting me, and during the examination of her chest, I observed, that the left breast was provided with two nipples, each of which gave milk, and were surrounded with a common areola. She assured me that her mother had exactly the same conformation.

Explanation of the Phenomena of Metallic Tinkling.—The tinkling, properly so called, or that produced by a bubble, (bullaire,) is produced by the breaking of a bubble of air in an effusion into the thorax, or into a cavity whose parietes are endowed with metallic resonance. The formation of the bubble conveys the idea that it must have passed through some liquid to arrive at the attending gas, and by a fistula opening into the liquids, through which the air that is introduced assumes the form of a bubble by its momentary submersion. However, we have seen in one of the observations quoted by Laennec, that bubbles of air had developed themselves by exhalation from the surface of the effused liquid. But it is very rare that in such cases they should give a metallic tinkling on breaking, because the cavity which contains them not having any communication with the bronchi, will not present a metallic resonance. From what I have said, one can understand that the bubble of air may come from different sources, since fistulas present many relations in their communication. Thus,

in the great majority of cases, the bubble is owing to the entrance of air from a bronchial fistula, into the effused fluid and below its surface. At other times the fistula does not terminate in the liquid, but in a quantity of purulent matter, which separates it from the gaseous expansion, so that the air in traversing the fistula raises in its passage the purulent matter, in the form of bubble, and this breaking high above the effused fluid, still produces the metallic effect. Finally, we may admit that these tinkling bubbles may, as I have already remarked, be produced by the exhalation of gas at the surface of an effused fluid, without any bronchial communication, and that they may sometimes result from the introduction of gas into the pleural cavity, by means of a communication from the stomach or intestines. Of all these various sources of bubbles, the one which occurs most frequently, and that which gives the most positive results is the first, namely, that arising from air insinuating itself, through a fistula, from the lung into the effused fluid; it is of that I intend to speak more particularly in the following pages.

The bubble will be single or it will consist of the union of several bubbles, according to the unity or multiplicity of the several tubes that may form it: its volume will be in a ratio corresponding with the diameter of the fistula. Bubbles may burst as soon as they arrive at the surface of the fluid; at other times, when that shall be thick or viscous, they may remain unbroken for some time, until an accidental motion of the chest cause their rupture: this accidental motion of the chest is not to be looked on as the cause of the tinkling.—*Beau. Archives Generales de Medicine*, vol. iv. March, 1834.

Electricity for the Cure of Warts, by Albert G. Welch, of Annapolis.—Having had during the past winter, when I attended lectures on chemistry, frequent opportunities of trying experiments with electricity, and having several warts on my hands, to get rid of which I was exceedingly anxious, and having tried repeatedly the efficacy of nitrate of silver, and other caustics, without any advantage, I determined to try the efficacy of electricity. I therefore commenced by sending sparks through them, which was repeated for five minutes daily, for five days, when to my great satisfaction, I found that they had entirely disappeared, since which time they have not re-appeared, which they did when removed by the knife, or by caustic.

As there may be others who would like to be clear of such disfiguring excrescences, and as the mode of removing them may not be generally known, if you think it worth publishing, I would be glad if you would insert the same in your journal.—*Baltimore Med. and Surg. Journal*, Oct., 1833.

Relative Mortality of different Countries

Countries,	Periods or Epochs.	Average number of Deaths.	Ratio of the average of Population.	Annual mortality in each million of inhabitants.
Sweden and Norway,	1821 to 1825	79,900	1 in 47 inh.	21,300
Denmark,	1819	33,800	1 ... 45 ...	22,400
European Russia,	1826	960,000	1 ... 44 ...	22,700
Kingdom of Poland,	1829	98,000	1 ... 44 ...	22,700
Britannic Islands,	1818-1821	378,000	1 ... 65 ...	18,200
Netherlands,	1827-1828	162,000	1 ... 38 ...	26,500
Germany Proper,	1825-1828	290,000	1 ... 45 ...	22,400
Prussia,	1821-1826	303,500	1 ... 39 ...	25,600
Austrian Empire,	1828	675,000	1 ... 40 ...	25,000
France,	1825-1827	808,200	1 ... 39 ...	25,600
Switzerland,	1827-1828	50,000	1 ... 40 ...	25,000
Portugal,	1815-1819	92,000	1 ... 40 ...	25,000
Spain,	1801-1826	307,000	1 ... 40 ...	25,000
Italy,	1822-1828	660,000	1 ... 30 ...	33,300
Greece,	1828	33,000	1 ... 30 ...	33,300
Turkey in Europe,	1828	334,800	1 ... 30 ...	33,000
Northern Europe,	. . .	2,972,100	1 ... 44 ...	22,700
Southern ditto,	. . .	2,284,200	1 ... 36 ...	27,800
TOTAL,	. . .	5,256,300	1 ... 40 ...	25,900

According to this table, and many others of a more detailed nature, there annually dies :

1 inhabitant in 28 in the Roman State, and the ancient Venetian provinces; 1 in 30 in Italy in general, Greece and Turkey; 1 in 39 in the Netherlands, France, and Prussia; 1 in 40 in Switzerland, the Austrian empire, Portugal and Spain; 1 in 44 in European Russia and Poland; 1 in 45 in Germany, Denmark, and Sweden; 1 in 48 in Norway; 1 in 53 in Ireland; 1 in 58 in England; 1 in 59 in Scotland and Iceland.

These numbers present the following results:

The smallest chances of life and its shortest duration are not, as one might believe, amongst the effects of the rigorous climate of Norway, or the marshy soil of Ireland; it is in the beautiful climate of Italy that life is reduced to its minimum extent. On the contrary, it is among the icy rocks of Iceland, in the midst of the eternal fogs of Scotland, where man attains his greatest age.

Of all the European States, the British Isles are, in this respect, the most favoured; of each million of inhabitants, they only lose 18,200, whilst the mortality is almost double in the countries washed by the Mediterranean.

Life, next to these, is most certain in Norway and Sweden. *Cæteris paribus*, three people die in the south of Europe, two only and

hardly that, die in ancient Scandinavia. Denmark and Germany enjoy, as it were, similar advantages.

Russia and Poland, where nature and fortune have not been lavish of the necessities of life, enjoy, however, an astonishing longevity. The population, comprising a mass of nearly sixty millions, spin out their existence almost one half longer than that which is enjoyed by the inhabitants of Italy; and exactly twice the length of time which any one can expect to live in the capital of Austria.

The average time of life which only cuts off one victim in 40 annually, is to be found in the Cantons of Switzerland, the Austrian Provinces, and in the Spanish Peninsula, in consequence of the sun and the dryness of the air.

France, the Netherlands, and Prussia, nearly reach the same term, and will soon get beyond it, unless war or some other scourge arrests the progress of their social amelioration.

In the rest of Europe, the mortality amounts to one-thirtieth of the population, and is even frequently increased by accidental causes, which have for a long time endangered the prosperity of the shores of the Mediterranean.

On the whole, we reckon, one year with another, 5,256,000 deaths in 210 millions of people, by a mortality of one-fortieth part, which is unequally distributed among the northern and southern states. The former have only one death in 44, the latter, one in 36 persons. Of one million of inhabitants in districts situated in the north of France, 22,700 die; 27,000 die in those which lie towards the south. This is a difference of more than 5,000 deaths, equivalent to a two-hundredth part of the population.

If we carefully examine these numbers and those of the tables of details from which they are extracted, we will discover that two great causes which predominate over all the others, determine the ratio of mortality to the population, or, in other words, regulate the number of the chances of human life. These are the influence of climate and civilization.

The climate is peculiarly favourable to the prolongation of human life when it is cold or even rigorous, or when the humidity in the environs of the sea is combined with a low temperature.

The smallest mortality in Europe, occurs in maritime countries which are in the vicinity of the polar circle, such as Sweden, Norway, and Iceland. This is also the case in countries where, as in Russia, the influence of climate is not aided by civilization, and of itself is sufficient to assure long life to mankind.

Countries where the heat is moderate, are not, as might be supposed, among those which possess the advantage of a small mortality; to obtain it, they must acquire the benefits of a high state of social order.

The southern countries, the mild climate of which seems to be so favourable to the human race, are, on the contrary, regions where life is exposed to the greatest dangers. In the stalling plains of Italy,

the chances of dying are one-half greater than those of cold and foggy Scotland; and under the beautiful skies of Greece, the certainty of life is one-half less than among the ice of Iceland.

The places of the torrid zone, the mortality of which has been calculated, show the pernicious influence which is exercised over the existence of mankind by a high temperature.

Latitude	Place	Deaths in 1000	Source
6° 10'	Batavia	1 in 26	Official documents.
10° 10'	Trinidad	1 in 27	Pugnet.
13° 54'	St. Lucia	1 in 27	M. de J.
14° 44'	Martinique	1 in 28	M. de J.
16° 59'	Guadaloupe	1 in 27	M. de J.
18° 56'	Bombay	1 in 30	Transactions Acad.
23° 11'	Havanagh	1 in 33	Humboldt.

The resistance of the vital principle in the tropics differs according to the races of men; and its duration in some places is, for the one, double or triple that for the others. The following are examples of this variation:

Batavia, in 1805—Europeans, 1 in 11 individuals; Slaves, 1 in 13; Chinese, 1 in 29; Javanese, 1 in 46. Bombay, in 1816—Europeans, 1 in 18; Mussulmen, 1 in 17; Parsees, 1 in 24. Guadaloupe, 1816 to 1824—Whites, 1 in 23; Freedmen, 1 in 35. Martinique, in 1815—Whites, 1 in 24; Freedmen, 1 in 38. Grenada, 1811—Slaves, 1 in 22. St. Lucia, in 1802—Slaves, 1 in 20.

We may compare this immense mortality in the torrid zone, with that which occurs in Madeira, the only colonial establishment in the temperate zone. Heberden has calculated, that the deaths in that island were in the proportion of 1 to 49.89, of the whole inhabitants.

The effects that the degree of perfection, more or less extensive, of the social economy exerts on mortality, are not less extensive than those the cause of which is to be found in the influence of the climate.

The influence of the progress of civilization is discovered by comparing the ratio of the deaths to the population of the same country at epochs, the intervals of which have been marked by social ameliorations. The following series of universal terms presents an instructive comparison:

The number of deaths compared with that of the inhabitants was,

In	Period	Ratio	Period	Ratio
In Sweden,	1754 to 1763	1 in 34	in 1821 to 1825	1 in 45
— Denmark,	1751 to 1756	1 — 32	— 1819	1 — 45
— Germany,	1788	1 — 32	— 1825	1 — 46
— Prussia,	1717	1 — 30	— 1821 to 1826	1 — 39
— Wurtemberg,	1749 to 1754	1 — 32	— 1825	1 — 45
— Austria,	1822	1 — 40	— 1828 to 1830	1 — 43
— Holland,	1800	1 — 26	— 1824	1 — 40
— England,	1690	1 — 33	— 1821	1 — 58
— Great Britain,	1785 to 1789	1 — 43	— 1800 to 1804	1 — 47
— France,	1776	1 — 26	— 1825 to 1827	1 — 39
— Canton of Vaud,	1756 to 1766	1 — 35	— 1824	1 — 47
— Lombardy,	1769 to 1774	1 — 27	— 1827 to 1828	1 — 31
— Roman States,	1767	1 — 24	— 1822	1 — 28
— Scotland,	1801	1 — 44	— 1821	1 — 59

Thus the mortality has diminished,—

In Sweden, nearly $\frac{1}{4}$ in 61 years; in Denmark, $\frac{1}{4}$ in 66 years; in Germany, $\frac{1}{4}$ in 37 years; in Prussia, $\frac{1}{4}$ in 106 years; in Wurtemberg, $\frac{1}{4}$ in 73 years; in Austria, $\frac{1}{4}$ in 7 years; in Holland, $\frac{1}{4}$ in 24 years; in England, $\frac{1}{4}$ in 131 years; in Great Britain, $\frac{1}{4}$ in 16 years; in France, $\frac{1}{4}$ in 50 years; in Canton of Vaud, $\frac{1}{4}$ in 64 years; in Lombardy, $\frac{1}{4}$ in 56 years; Roman States, $\frac{1}{4}$ in 62 years.

For thirty years, the mortality has been stationary in Russia and Norway; it has increased in the kingdom of Naples.

On the whole, there has been, for 80 years, a mortality of 1 individual in 36 throughout Europe, according to Süssmilch; our calculations do not exceed 1 in 40, according to the average of later years. On the continent of Europe there has, therefore, been a diminution of one-ninth in the mortality of the aggregate of the people, if we can rely on the German writer on statistics. We are, however, inclined to think, that, during his time, the mortality generally was less than one-thirtieth of the population, which supposes that it is more than one-third less at present in proportion to the augmentation of its population.

This gradual diminution of mortality arises from the same causes in the principal towns in Europe. The number of deaths compared with that of the inhabitants at distant epochs, gives the following proportions:

Paris,	in 1650	1 in 25 inhab.;	in 1829	1 in 32
London,	— 1690	1 — 24 —	— 1828	1 — 55
Berlin,	— 1755	1 — 28 —	— 1827	1 — 34
Geneva,	— 1560	1 — 18 —	— 1821	1 — 43
Vienna,	— 1750	1 — 20 —	— 1829	1 — 25
Rome,	in 1762–1771	1 — 21 —	— 1828	1 — 31
Amsterdam,	in 1761–1770	1 — 25 —	— 1828	1 — 29
Cambridge,	in 1811	1 — 41 —	— 1821	1 — 58
Norfolk,	— 1811	1 — 50 —	— 1821	1 — 59
Manchester,	— 1757	1 — 25 —	— 1821	1 — 58
Birmingham,	— 1811	1 — 30 $\frac{1}{2}$ —	— 1821	1 — 43
Liverpool,	— 1773	1 — 27 —	— 1821	1 — 41
Portsmouth,	— 1800	1 — 28 —	— 1811	1 — 38
Petersburg,	— 1768	1 — 28 —	— 1828	1 — 48
Stockholm,	in 1758–1763	1 — 19 —	— 1827	1 — 26

The annual mortality has also diminished in Paris, more than $\frac{1}{4}$ in 80 years; in London, more than $\frac{1}{4}$ in 178 years; in Berlin, $\frac{1}{4}$, or nearly $\frac{1}{2}$, in 72 years; in Geneva, $\frac{1}{4}$ in 261 years; in Vienna, $\frac{1}{4}$ in 80 years; in Rome, $\frac{1}{4}$ in 63 years; in Amsterdam, $\frac{1}{4}$ in 64 years; in Cambridge, $\frac{1}{4}$ in 10 years; in Norfolk, $\frac{1}{4}$ in 10 years; in Manchester, $\frac{1}{4}$ in 64 years; in Birmingham, nearly $\frac{1}{4}$ in 10 years; in Liverpool, $\frac{1}{4}$ in 38 years; in Portsmouth, more than $\frac{1}{4}$ in 11 years; in Petersburg, nearly $\frac{1}{4}$ in 40 years; in Stockholm, more than $\frac{1}{4}$ in 67 years.—*Edinburgh New Philosophical Journal*, April, 1834.

Ricord's Treatment of Ulceration of the Os Tincæ.—In some patients who have exhibited ulceration of the os tincæ running

into the cavity of the womb, and of whom the uterine secretions have been purulent, I have attempted canterization of the internal surface of this cavity. To this end, I have had constructed a syringe with a double cylinder, of which the pistons worked singly, and which enclosed in one of its cylinders acidulous nitrate of mercury, diluted with twelve parts of distilled water, and in the other pure water. Its pipe, also double, is adapted to a gum elastic catheter, of about eight inches in length, which, opened at its two extremities, and smeared with a greasy body, is introduced into the uterus. The injection of the acidulous nitrate is then pushed gently, and in small quantity, to the extent of about a small tea-spoon full, and after having allowed it to remain for a minute or two, the watery injection is then pushed on. Five patients affected with very abundant purulent uterine discharges, and which had resisted all other means, have been thus treated and cured in my clinical service at the Hôpital des Vénériens. I should state that I have only employed this means as a desperate remedy, and with all possible precautions, and that although I have never had any troublesome cases, yet in some patients it has produced accidents of short duration, but formidable in appearance, having determined in the greater number, instantaneous and very violent hysterical attacks; which, by the way, would tend to support the opinion of those who place the seat of that disease in the womb.—*London Medical and Surgical Journal*, May 24.

SURGERY.

New Hernial Bandages, by Messrs. Sanson and Cresson. Eolian Pads.—In the Royal Academy of Medicine, sitting of the 4th March, M. J. Cloquet read, in his own name and that of Messrs. P. Dubois and Harvez, of Chégoïn, a report on these bandages.—The principal inconvenience of all hernial bandages arises from the substance which enters into the composition of the pad, and which does not permit it to exercise at all times, a gentle, uniform, constant, and graduated pressure, the only conditions to be fulfilled in the treatment of herniæ. All the substances employed until now have the inconvenience of becoming wrinkled, hardened, &c. Messrs. Cresson and Sanson happily had the idea of fabricating the pads with Indian rubber; they have imagined three kinds:

1st. Pads all of Indian rubber.

2d. Hollow pads, and filled with a quantity of air, which can be augmented or diminished, which they call Eolian pads, with fixed air.

3d. Pads similar to the former, with this difference, that the air can be let out or introduced at pleasure, and in different quantity: Eolian pads with moveable air.

The exterior of all these pads is Indian rubber. The authors have thought proper to cover them with gold leaf, which in adding to

the appearance and neatness, does not interfere with its other essential qualities. These pads, independent of the advantages of elasticity, softness, &c., by their nature alone, are capable of any alteration, however long they may be used. Experience has already confirmed their superiority over the ancient ones.—*Revue Médicale*.

Re-union of a Portion of the Thumb entirely separated.—M. Beau, intern at Salpêtrière, observed this case, the principal points of which he recapitulates in the following manner:—1st, September 7th, 1833, the extremity of a thumb entirely divided about the centre of the nail, after having been separated from the body for more than half an hour, was replaced and maintained by straps of adhesive plaster. 2d. The 15th, that is, eight days after, the extremity was found re-united by an adhesion, which, although weak on that day, was much more solid on the 18th. 3d. The nail and epidermis separated from the subjacent parts, and leaving denuded the end of the phalanx necrosed, retained in its place by a slight eschar adhering to the soft parts, which alone are vitally re-united to the corresponding soft parts of the finger. 4th., 21st. The necrosed bone removed: the eschar, not detached until the 23d, cicatrization complete on the 27th, twenty days after the separation of the extremity of the thumb, and six days only after the first appearance of pus.*—*Ibid*.

Caries of the Spine, Anatomical Society of Paris.—M. Gendron found the following alterations on the body of a child ten years of age, who died from disease of the spine. The body of the second cervical vertebra was completely denuded, and the intervertebral substance between that and the third vertebra entirely destroyed; a large purulent collection surrounded the bones and carried forward the anterior muscles of the neck, and on the side, the aponeurotic sheath of the long muscles of the neck, the muscular part of which was destroyed: posteriorly, the sinus communicated with the vertebral canal between the two vertebræ, above mentioned, and extended to the basilar cavity (*gouttière basilaire*), raising the dura mater and occipito-axoïdien ligament. The lumbar region presented also a caries with a purulent accumulation.—*Ibid*.

Cancer of the Diploë.—A female subject with cancer of the breast, in whom the two thigh bones were on one occasion fractured by a very slight cause, was presented to M. Tessier, with cancerous tumours developed in the interior of the medullary canal of each femur; the sides of the osseous cylinder were reduced to an extreme thinness and fragility. The bones of the cranium presented also several cancerous tumours at different stages of development, but which all appeared to have their origin in the diploë.—*Ibid*.

* See our last Number for a case of reunion of a portion of the index finger.—Ed.

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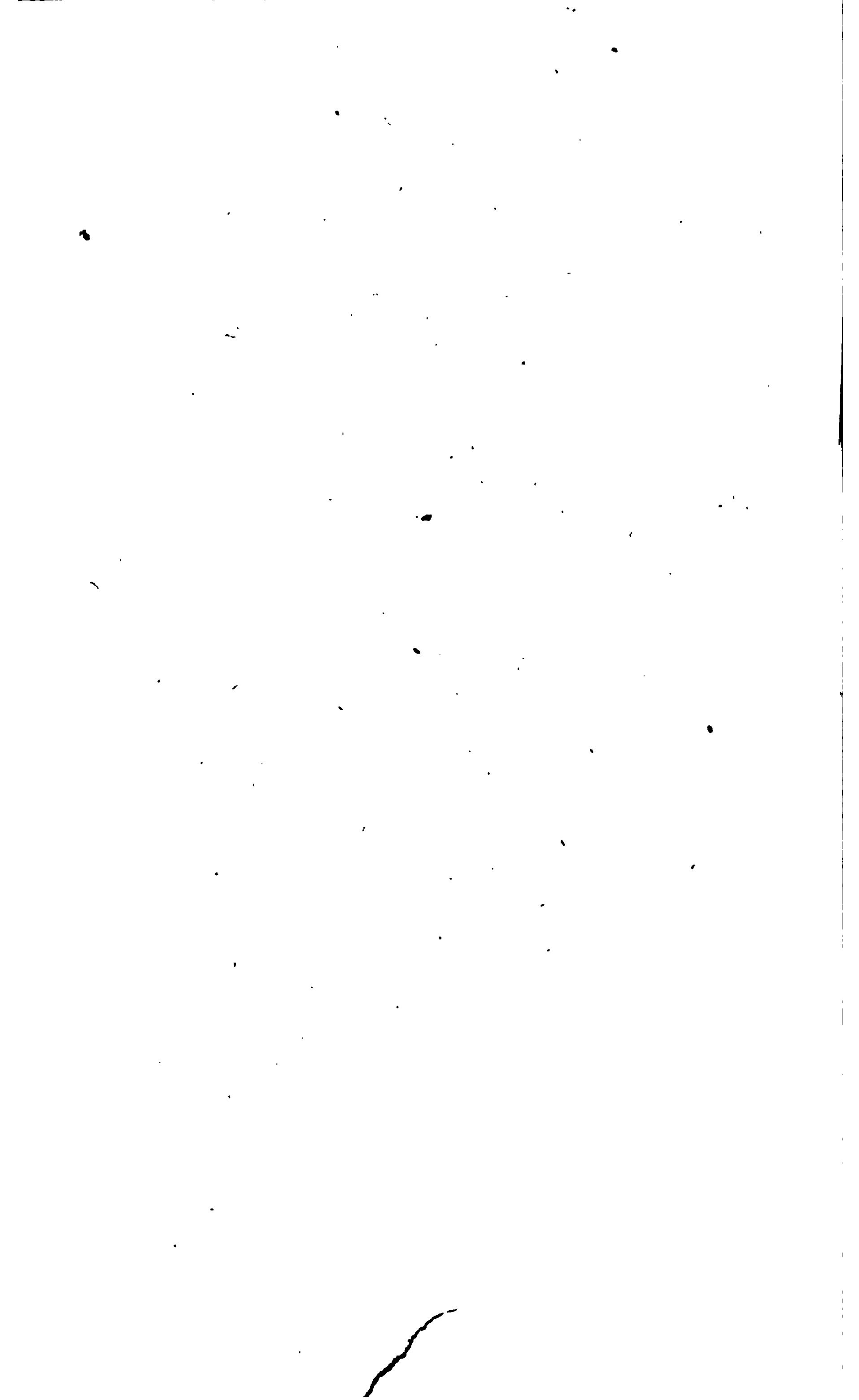
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